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The American Historical Review

THE AMERICAN HISTORICAL ASSOCIATION

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there is
strength*

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† Mr. Appleby died on December 19, 1974. An obituary will appear in the April issue. The staff of the *AHR* wish to record here the immense debt they—and the historical profession—owe to him for his wise and meticulous administration of the book reviewing operation and for his painstaking compilation of the indexes.

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American Historians and the World Today: Responsibilities and Opportunities

LEWIS HANKE

NATIONS HAVE LONG HAD RELATIONS with each other and have acknowledged some responsibilities to each other in the world, but have historians? Members of the American Historical Association will increasingly ponder this question as the time approaches for the first meeting in the United States of the International Congress of Historical Sciences. Some American historians have attended the other international meetings held in Europe since 1900, but the congress is expected to bring together in San Francisco in August 1975 several thousand historians, most of them Americans. The participants will read or listen to learned papers on the "grand themes of history" as well as on a large number of smaller topics, will attend receptions, and will enjoy the still powerful attractions of northern California. One may well ask to what useful end all this movement, all this expense of time and money will be directed.

My answer is a simple one. International congresses of historians do not fully meet the needs of the times and cannot be expected to do so unless the organization that sponsors them is substantially changed and unless national organizations accept far greater international responsibilities. For the AHA this means that we need to strengthen the teaching and writing in the United States of the history of all regions of the world, to recognize the increasingly significant study abroad of our history, and to foster in all possible ways the professional relations of historians on an international scale. For the ICHS to meet its challenge, this largely Western organization must review its traditional operations in various specific ways, which will be suggested later.

This may seem a Utopian proposal to those aware of the political problems encountered by the congresses and to historians everywhere who are

This is a presidential address delivered by Mr. Hanke at the annual meeting of the American Historical Association, Chicago, December 28, 1974. The author wishes to acknowledge the valuable assistance received from many persons, beginning with the discussion held with a Spanish railroad track walker while waiting in the Escorial station for a train to Madrid in the summer of 1929.

often concerned principally with their own history. Conor Cruise O'Brien wrote:

Most history is *tribal* history: written . . . in terms generated by and acceptable to, a given tribe or nation. . . . Historians, like other people, tend to identify with a community—not necessarily the one in which they were born—and in the case of modern historians this identification is likely to affect, and interact with, the character of their work, their career, their geographical location, and their public. Normally they write within a convention which suggests these conditioning factors do not exist, or can be ignored. Marxist historians, indeed, emphasize such factors but only as limitations on bourgeois historians.¹

If this be true, or partially true, why should Americans concern themselves with the history of other tribes and with other tribal historians?

MEMBERS OF THAT LARGE and diverse tribe which inhabits what is called the Western World can best begin to examine these questions by considering the consequences of the discovery of America on the writing of history. Herbert Butterfield has emphasized that one of the unique characteristics of the West is its "historical mindedness" and that history only in modern times has become the kind of subject it is today.² Yet he and many others ignore Iberian influences, a considerable omission because in the development of history since 1492 Spain was in the forefront, at least chronologically, of all European nations, and Portugal also made significant contributions.

Historians should be grateful for the Spaniards' keen sense of the past and for their almost unconscious though certainly widespread realization that Spanish actions overseas would one day be scrutinized by posterity. Columbus started the practice of writing about America, and many followed his example, for the conquest so stimulated their imagination that they came to look upon it as the greatest event since the coming of Christ. Even as the conquistadores roamed over vast areas of land and sea and missionaries attempted to Christianize millions of Indians, they collected historical materials and composed chronicles on a monumental scale.³ This copious documentation constitutes another kind of treasure from the Indies, distinct from the gold and silver found there, a documentation that still excites historians by its richness and depresses them by its quantity, for every fleet from Spanish America carried homeward thirty or forty boxes of documents, often carefully indexed for convenient study by the council of the Indies.⁴

¹ Conor Cruise O'Brien, *States of Ireland* (New York, 1972), 16–17.

² Herbert Butterfield, *Man on His Past: The Study of the History of Historical Scholarship* (Boston, 1960), vii–xi.

³ Lewis Hanke, "The Other Treasure from the Indies during the Epoch of Emperor Charles V," in Peter Rassow and Fritz Schalk, eds., *Karl V: Der Kaiser und seine Zeit* (Cologne, 1960), 94–103.

⁴ Juan Manzano, ed., "Un documento inédito relativo a como funcionaba el Consejo de Indias," *Hispanic American Historical Review*, 15 (1935): 316.

Beginning with Viceroy Antonio de Mendoza, who arrived in Mexico in 1535, the principal Spanish officials manifested a keen interest in history. Mendoza wanted to know about the "chronicles, hieroglyphs, and pictures from Montezuma's palace which told of the migrations of the ancient Mexicans." Many other viceroys, moreover, commissioned the writings of histories or received histories voluntarily written by Spaniards on American subjects.⁵ Sometimes there was a polemic purpose, as when Viceroy Francisco de Toledo organized in the 1570s a study of Inca history to prove Spain's contention that her conquest not only had followed just principles but in fact had liberated the Indians from a tyrannical and unjust Inca rule. But even this stern official was much impressed by what he saw in Peru, and he proposed that a museum be created in Spain where "Indian art and the products of nature" in America could be studied.⁶

Ecclesiastics were eager to have their missionary triumphs recorded. In 1536 the Franciscan chapter in Mexico City recommended that one of their number write an account of Indian life in pre-Spanish days as well as a history of the labors of the first group of Franciscans, known as "The Twelve Apostles," from the time of their arrival in 1524.⁷ The dedicated missionaries Spain sent to America were convinced that the discovery and conquest not only afforded a unique opportunity to bring the Gospel to the Indians but also, according to some, foreshadowed the rapid approach of the end of the world and the coming of the millennial kingdom. Though the traditional Church was being destroyed in Europe, or at least severely challenged by Luther, the friars were determined that a new and more powerful Church be built in America. But there was no time to be lost. Faced with an enormous diversity of native languages, which were in turn divided into hundreds of dialects, all phonetically and morphologically alien to European languages, the early friars first tried to learn Nahuatl by playing with Indian children to acquire useful phrases. Frustrated in their attempt to identify even a few words but unwilling to allow one Indian soul to suffer damnation because of their own ignorance, some of the early friars preached to the Indians in Latin or Spanish in the hope that Christian fervor would make up for linguistic deficiencies.⁸

As the conquest proceeded and Philip II increasingly came to dominate the administrative machinery governing the far-flung Spanish empire, a demand arose for an adequate history of Spanish accomplishments as a whole. A decisive epoch for historiography began about 1570 when the council of the Indies decided that good administration required an archive containing organized information on previous laws and past events, ma-

⁵ George P. Hammond and Agapito Rey, eds., *Obregon's History of 16th Century Explorations in Western America* (Los Angeles, 1928), 10-11.

⁶ Marcos Jiménez de la Espada, *Tres relaciones de antigüedades peruanas* (Madrid, 1879), xix.

⁷ Francis Borgia Steck, ed., *Motolinia's History of the Indians of New Spain* (Washington, 1951), 20.

⁸ David Haberly, "The Hieroglyphic Catechisms of Mexico" (B.A. thesis, Harvard College, 1963), 3.

chinery for obtaining current reports, and an official historian.⁹ A detailed questionnaire was drawn up, which every governor in America was ordered to answer with specific data on the history, people, climate, and geography of the territory he administered. Begun as a brief inquiry in 1569, this questionnaire soon grew to fifty items and—since bureaucrats never seem to have enough information—eventually became a printed volume of three hundred and fifty questions, which must have been a heavy cross for hard-pressed governors in the far reaches of the empire to bear.¹⁰

The first historian was appointed in 1573, and beginning in 1578 instructions were regularly sent out requiring the principal royal representatives in America to search their archives for historical manuscripts and to dispatch the originals or authentic copies to the council of the Indies so that a true, general history of the Indies could be written. The council had a realistic view of the habits of historians, for it decreed that the appointee would not receive the last quarter of his salary until he had turned in some completed text. For almost two hundred and fifty years, until the eve of independence, Spain sent out a constant stream of orders for information and history.¹¹

Controversy inevitably developed over what constituted “true” history. To set straight the record as he saw it, one foot soldier of Ferdinand Cortez, Bernal Díaz del Castillo, composed a *True History of the Conquest of New Spain*, now a classic on the discovery period.¹² Bitter and prolonged battles on the justice of Spanish dominion and the place of Indians in Spanish society produced an enormous amount of historical documentation, which continues to attract historians. We are particularly aware of these disputes today because 1974 witnessed the commemoration of the five-hundredth anniversary of the birth of Bartolomé de Las Casas, the best-known defender of the Indians and a persistent doubter of the justice of Spanish rule. Inasmuch as my volume on his doctrine has recently appeared, I will restrain myself, with some difficulty, from analyzing his role in the development of historical writing in America and his insistence that the American Indians should not be considered natural slaves according to the Aristotelian doctrine but instead should be persuaded by peaceful methods to accept the Christian faith.¹³ To prove that the Indians were not semianimals whose

⁹ Rómulo D. Carbia, *La crónica oficial de las Indias Occidentales: Estudio histórico y crítico acerca de la historiografía mayor de Hispano-América en los siglos XVI a XVIII, con una introducción sobre la crónica oficial en Castilla* (Buenos Aires, 1940).

¹⁰ Marcos Jiménez de la Espada, *Relaciones geográficas de Indias*, ed. José Urbano Martínez Carreras, 1 (Madrid, 1965): 5–117. For an exhaustive description and evaluation of these reports, see Howard F. Cline, “Guide to Ethnohistorical Sources: Part One,” in Robert Wauchope, ed., *Handbook of Middle American Indians*, 12 (Austin, 1972): 183–242, 324–95.

¹¹ Sylvia Vilar, “Une vision indigéniste de l’Amérique en 1812,” *Mélanges de la Casa de Velázquez*, 7 (Paris, 1971): 339–401.

¹² Bernal Díaz del Castillo, *True History of the Conquest of New Spain*, ed. and tr. Alfred Percival Maudslay (London, 1908–16).

¹³ Hanke, *All Mankind Is One: A Study of the Disputation between Bartolomé de Las Casas*

property and services could be commandeered at will by the Spaniards, Las Casas prepared a large work entitled *Apologetic History*, in which he advanced the idea that the Indians compared very favorably with both the Spaniards and the peoples of ancient times, were eminently rational beings, and in fact fulfilled every one of Aristotle's requirements for the good life.

The main argument of Las Casas against those who considered the Indians less than human beings, an argument that entitles him to be included as a principal member in that great tribe that might be called "all mankind," may best be summarized in his own words:

Thus mankind is one, and all men are alike in that which concerns their creation and all natural things, and no one is born enlightened. From this it follows that all of us must be guided and aided at first by those who were born before us. And the savage peoples of the earth may be compared to uncultivated soil that readily brings forth weeds and useless thorns but has within itself such natural virtue that by labor and cultivation it may be made to yield sound and beneficial fruit.¹⁴

We see here the beginning of the great dispute today, in which William Shockley and Arthur Jensen contend that blacks are born with genetic deficiencies that limit their intellectual growth and hinder their attempts to compete with whites.

The history of the relations between Europeans and natives in the conquest period is rich in detail. Indian men loved to wear their hair long, which offended Spaniards, whose custom was to have their hair cut short. Besides, the Spaniards said long hair was filthy and that Indian women usually slept with the men whose hair they braided, which was an offense to Christian morals.¹⁵ In Manila one zealous sixteenth-century bishop was so opposed to allowing Chinese converts there to keep their queues that it required an order from the council of the Indies to stop him from cutting them off. Instead, the bishop and his missionaries were ordered to treat the Chinese "with prudence and intelligence, and with the kindness and mildness required to nurture such new and tender plants."¹⁶ In the following century Jesuits in the Philippines denounced the drinking of chocolate; in Mexico they said it was a danger to chastity for it aroused the passions. By the end of the century, however, the nutritious drink had become a standard breakfast food on Jesuit tables in Spain and the Indies.¹⁷

But one custom of the Indians was not accepted—human sacrifice by the

and Juan Ginés de Sepúlveda in 1550 on the Intellectual and Religious Capacity of the American Indians (De Kalb, 1974).

¹⁴ Bartolomé de Las Casas, *Apologética historia sumaria*, ed. Edmundo O'Gorman, 1 (Mexico, 1967): 258.

¹⁵ Juan de Matienzo, *Gobierno del Perú* (1567), ed. Guillermo Lohmann Villena (Paris, 1967), 80.

¹⁶ Philip II to Bishop Salazar, June 23, 1587, Archivo General de Indias, Seville, Filipinas 339, bk. DDI, pt. 2, fol. 155v.

¹⁷ Horacio de la Costa, S.J., *The Jesuits in the Philippines, 1581-1768* (Cambridge, Mass., 1967), 249.

Aztecs. None of the many contemporaries of Las Casas who prepared histories of the dramatic meeting of the West with Indian culture supported his view that the practice of human sacrifice, which revolted lay and ecclesiastics alike, should be understood in the light of the Indians' own history and doctrines. Las Casas discerned, underneath the horrible and bloody aspects of these rites, a commendable spirit of religious devotion that might be directed to higher ends and enlisted in the service of the only true God.¹⁸

As the conquest proceeded and as the archives of the council of the Indies in Spain began to fill, Spaniards gave more and more attention to Indians and their culture. What taxes had they paid to their rulers before the Spaniards came? What religious concepts did they have that must be rooted out to prepare them for the true faith? Did their previous habits indicate that they were capable of becoming civilized and Christian? Though ecclesiastical writers concentrated on religious aspects of the conquest, they also viewed it in the round; they wrote on art and cooking, child training, disease and death, and the many other subjects that interested them.

The greatest single figure in the study of Indian cultural history was the Franciscan, Bernardino de Sahagún. One of the earliest missionaries in Mexico, he was not satisfied with the approach involving playing with children and almost at once began to study Nahuatl and collect materials bearing on the Indian past. In 1547 his superior ordered him to work on a history, and for a decade he continued his investigations. Then in 1558 he embarked in Tepepulco near Mexico City upon a large-scale, systematic study of Aztec culture, with the aid of several of his own Spanish-speaking Indian disciples who also knew Latin. Sahagún had written down many extensive lists of items—culture elements they would be called today—on which he desired information, and he brought together about a dozen old men reputed to be wise in their own lore. Sahagún and his research assistants interrogated these informants during 1558–59; it was the first oral-history project in America. The old men illustrated their replies by preparing a series of drawings and paintings, which were explained in writing by the Indian assistants. These visual materials became an essential part of the historical documentation.

After two years of discussions with the old men and his young Indian assistants at Tepepulco, Sahagún moved to another center at Santiago Tlatelolco to test his preliminary findings, for he exhibited the fundamental skepticism of the historian who is rarely satisfied that he has complete or accurate sources. For two more years, 1560–61, he reviewed and revised all his material with the help of a new set of informants. It took him three more years to re-edit the whole manuscript, which was still in Nahuatl, and to rework it into twelve books, each one broken down into chapters and each chapter into paragraphs.

The result was a carefully organized mass of text and 1,850 illustrations

¹⁸ Hanke, *All Mankind Is One*, 93–95.



Musicians and entertainers (bk. 1, no. 19)



Bathing the baby (bk. 6, no. 29)

on the spiritual and material aspects of the life of the ancient Mexicans as the Indians remembered them. It was decidedly not, like so much of the transatlantic literature of the period, a European view masquerading as a description of far-off peoples, but a remarkable collection of oral literature that expressed the soul and life of the Aztec people at the time of their greatness, one of the finest sources known for ethnohistory.¹⁹ While some other Spaniards were fanatically destroying Indian culture, Sahagún methodically brought together documentation on the functions, ceremonies, legends, and traditions of the many gods of the Aztecs, on astronomy, astrology, the calendar, and the calculation of the recording of time, which was of great importance to them. Sahagún also included their superstitions, rhetoric, philosophy, ideas of mortality, songs to the gods, and hymns to the sun, the moon, the stars, and the wind. The ancient rulers received much attention, as did their merchants and judges. The education of the children in the home and school was treated, as well as information on botany, zoology, and the animal and plant life of Mexico, mineralogy, agriculture, the preparation and preservation of edible plants, sculpture, painting, melting of metals, the jeweler's trade, house building, the raising and care of domestic animals, road building, and temple construction. The final book described the conquest of Mexico as seen by the conquered.

Sahagún's purpose was clear: to learn all about the Indian language and culture in order to help him and the other missionaries in their conversion labor. Thus he included descriptions of the ways in which Indians got intoxicated for ceremonial reasons, for Sahagún maintained that missionaries must know all about the sins of the Indians in order to correct them, just as doctors must study disease.

¹⁹ Henri Baudet, *Paradise on Earth: Some Thoughts on European Images of Non-European Man*, tr. Elizabeth Wentholt (New Haven, 1965), vii; J. H. Elliott, "The Discovery of America and the Discovery of Man," *Proceedings of the British Academy*, 63 (1972): 1-27; Benjamin Keen, *The Aztec Image in Western Thought* (New Brunswick, 1971).



The ruler's war array (bk. 8, no. 77)



Cleaning the teeth (bk. 10, no. 161)

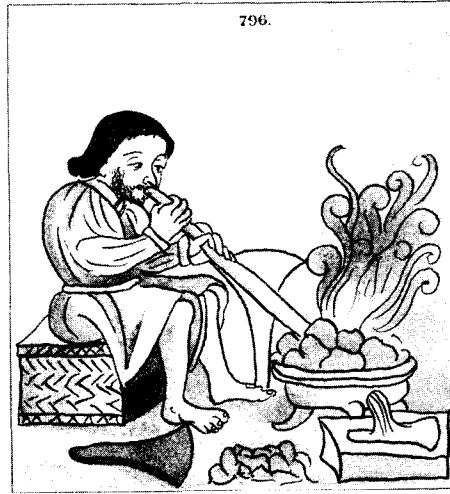
As Sahagún struggled through the years against obstacles and apathy he became so immersed in the study of Indian culture that he grew interested in it for its own sake and was concerned that contact with Europeans would cause the native culture to disappear or become hybridized. Thus there was dedication and urgency in his work. At last, as the result of a royal order in 1577 instructing Viceroy Enríquez Martínez to collect all of Sahagún's manuscripts for the council of the Indies, the Nahuatl text was translated into Spanish and sent to the council.

Sahagún died in 1590 without seeing a single chapter of his monumental work published. Only in recent years have complete editions of both the Nahuatl and the Spanish texts become available, based upon the various manuscripts dispersed in libraries in Florence, Madrid, and Mexico City. The first translation into any language of the entire Nahuatl manuscript has just been completed, after thirty-five-years' labor, by Charles E. Dibble and Arthur J. O. Anderson, whose English version, *General History of the Things of New Spain*, imparts the spirit as well as the substance of the original. This outstanding work of American scholarship, richly footnoted and based upon extensive researches by European and Mexican scholars as well as those of the editors, will enable the English-speaking world to appreciate one of the foundation works in the history of how scholars in one culture have studied another.²⁰

²⁰ Bernardino de Sahagún, *General History of the Things of New Spain*, tr. and ed. Arthur J. O. Anderson and Charles E. Dibble (Salt Lake City, 1950-69). The history and bibliography of Sahagún's work are extraordinarily complex. For a competent guide through the maze, see Howard F. Cline and John B. Glass, "Guide to Ethnohistorical Sources: Part Two," in Wauchope, *Handbook of Middle American Indians*, 13 (Austin, 1973): 186-239. This rich volume contains much information on other aspects of history writing in the Indies by such authorities as Ernest J. Burrus, Charles Gibson, and others. See also Munro Edmundson, ed., *Sixteenth-Century Mexico: The Work of Sahagún* (Albuquerque, 1974). For a general study of the growth of Spanish studies on Indian cultures during the conquest, see Carmelo Lisón Tolosana, *Antropología social en España* (Madrid, 1971), 1-96. See also Alfredo Jiménez Núñez, "La antropología y la historia de América," *Revista de Indias*, 107-08 (1967): 59-87; and Núñez,



Treatment of spider bites (bk. 11, no. 287)



Copperworking (bk. 11, no. 796)

The illustrations are from Bernardino de Sahagún, *Historia General de las Cosas de Nueva España*, 5 (Madrid, 1905). Photographs reproduced from the collection of the Library of Congress.

Sahagún must be recognized as one of the most complex Spaniards in sixteenth-century America. He was a member of a powerful nation, whose people believed themselves to have been singled out by God for His purposes just as certainly as the Puritans of Massachusetts Bay Colony were convinced that they were "God's Chosen People." He was a member of one of the most militant missionary nations that the world has ever seen, yet in an age when few persons displayed a respectful interest in any culture except their own he devoted many years of effort to understanding, from their viewpoint, practically all aspects of the life of the ancient Mexicans. For a sixteenth-century European, his was a remarkable achievement particularly when we realize that no other colonizing nation produced such a figure.²¹

"El método etnohistórico y su contribución a la antropología americana," *Revista Española de Antropología Americana*, 7 (1972): 163-96.

²¹ On the lack of French writers of Sahagún's stature, see Alfred Métraux, "Les precursseurs de l'ethnologie en France du XVI^e au XVIII^e siècle," *Journal of World History*, 7 (1962): 721-38. The early Jesuits in China had language difficulties. Donald F. Lach states that Matteo Ricci "was evidently the only one of the Europeans to learn more than a few polite expressions in Chinese." *Asia in the Making of Europe*, 1 (Chicago, 1965): 821. Even Ricci did not measure up to Sahagún, as will be seen from the study by George L. Harris, "The Mission of Matteo Ricci, S.J.: A Case Study of an Effort at Guided Culture Changes in the Sixteenth Century," *Monumenta Serica: Journal of Oriental Studies*, 25 (1960): 1-168. There was an impressive amount of information on Chinese culture available in Europe's major languages, according to Edwin J. Van Kley, "News from China: Seventeenth-Century European Notices of the Manchu Conquest," *Journal of Modern History*, 45 (1973): 561-82. But this information was not obtained by the rigorous methods of Sahagún. In India, according to Sir George B. Sansom, "it was not until 1606—after a hundred years of missionary effort—that the Jesuit father Roberto de Nobile, with the approval of the Society of Jesus, undertook a serious study of Hinduism in order to learn how it could best be criticized and confuted." *The Western World and Japan* (New York, 1950), 77. Though England had commitments in India from the beginning of the seventeenth century, Sir William Jones of the High Court in Calcutta was in 1783 the pioneer in the British study of Indian languages. The study of the Japanese language by the Jesuits Luís Frois and João

The work of Sahagún and other Spaniards who studied the history of Indian culture and the accomplishments of Spain in America have not yet sufficiently been analyzed or understood. Perhaps in 1992 when the five-hundredth anniversary of the voyage of Christopher Columbus across the Ocean Sea will be commemorated—and Spain has already appointed a commission to plan for this event—we will have an adequate examination of these works that helped to lay the basis for the modern study of history. Among the many figures who should appear in such a work, Sahagún will be seen not only as a “past glory” but as one whose work has significance for us today. As Miguel León-Portilla of the University of Mexico emphasizes, Sahagún’s supreme achievement is that he found a way to discover in a different culture those elements which are common to all mankind. León-Portilla concludes that the world today, with its many distinct cultures and physically closer together than ever before because of technological advances, needs the lesson of Sahagún, for it should help us to achieve relations with other cultures through dialogue and comprehension.²²

WHY DO SOME HISTORIANS in the twentieth century, which bears some striking resemblances to the time when Sahagún was at work, study other cultures? Why do many more historians, though occasionally attempting to develop professional relations on an international basis, continue to work only on the history of their own tribes?

Only fragmentary accounts have been published concerning the efforts of historians to create some kind of international community, which illustrates the truth of the remark by Charles Homer Haskins, “Many historians find it easy to be historically minded respecting everything save only history.”²³ Our best single source for an understanding of the development of the international congresses from the American viewpoint is the correspondence of J. Franklin Jameson, that giant among the founders of the

Rodrigues was on a fairly low practical level, states Tadao Doi, “A Review of Jesuit Missionaries’ Linguistic Studies of the Japanese in the 16th and 17th Centuries,” in Japanese National Commission for UNESCO, International Symposium on the History of Eastern and Western Cultural Contacts (1957), *Collection of Papers Presented* (Tokyo, 1959), 215–22. Their study was a far cry from the intensive linguistic effort of Sahagún who described his work as “a sweeping net to bring to light all the terms of this language, with their regular and metaphorical meanings, and ways of saying things.” Cline and Glass, “Guide to Ethnohistorical Sources: Part Two,” 203. On Europe’s abysmal record in studies of African cultures, see Katherine George, “The Civilized West Looks at Primitive Africa, 1400–1800: A Study in Ethnocentrism,” *Isis*, 49 (1958): 62–72. H. J. de Graaf remarks on how little research was done by the Dutch. “Aspects of Dutch Historical Writings on Colonial Activities in South East Asia with Special Reference to the Indigenous Peoples during the Sixteenth and Seventeenth Centuries,” in D. G. E. Hall, ed., *Historians of South East Asia* (London, 1961), 213–24. In his introduction Hall emphasizes the great strength of the “Europe-centricity of historians,” especially in the period before World War II (p. 8).

²² Miguel León-Portilla, “Significado de la obra de Fray Bernardino de Sahagún,” *Estudios de Historia Novohispana*, 1 (1960): 27.

²³ Charles Homer Haskins, “European History and American Scholarship,” *AHR*, 28 (1922–23): 225.

AHA, for it provides a running account of the activities of historians in the international meetings held since the first one in Paris in 1900. When Jameson attended the congress in London in 1913, the ignorance and the indifference of the European historians toward American history pained him. He reported that no one in Great Britain "was at all interested in American history." Nor did other European historians at the congress manifest the slightest curiosity in what had happened in the United States. By 1915 Jameson was fearful that World War I would create a state of mind "which for a long time will make it difficult for the students of history in various nations to come together in a spirit of harmony," and he was sufficiently realistic to see "only a restricted scope for international endeavor in history," due to "the fact that for the last four hundred years mankind has been chiefly organized in great states."²⁴ He did not expect European historians to cooperate much.

Although Jameson spent most of his life outside universities, he considered them the basis for sound historical activities. In 1919 he supported plans to establish a professorship of American history at the University of London. He also applauded the proposal that the 1923 congress should include one session devoted to our history; in fact, he wrote in a burst of chauvinism, "American history, between you and me, should be the chief pursuit of mankind henceforth."²⁵ The congress meeting in Brussels in 1923 was not prepared for such a radical step as a session on United States history alone but experimented with a separate session on "the history of the American continents," which may have reflected a reluctance to schedule a session on any subject that Europeans considered as parochial as United States history and on which they were not prepared to speak. The miscellaneous and scattered papers delivered at this session must have convinced the few Americans who attended that European scholars had little knowledge of or interest in our history.²⁶

Americans were sensitive in other ways too. Haskins devoted his presidential address in 1922 to recounting American contributions to European historiography, as if to make certain that everyone understood how much had been accomplished over here. He urged Americans not to be content with receiving European history secondhand, in packages prepared by European scholars, and insisted that American historians "participate fully and directly in all phases of the historical activity of our time." This question,

²⁴ J. Franklin Jameson to Andrew C. McLaughlin, Feb. 13, 1919, in Jameson, *An Historian's World: Selections from the Correspondence of John Franklin Jameson*, ed. Elizabeth Donnan and Leo F. Stock (Philadelphia, 1956), 230; Jameson to Waldo G. Leland, Mar. 24, 1924, in *ibid.*, 298. For some recent sober and detailed views on the continuing strength of nationalism, see Boyd C. Shafer, "Webs of Common Interests: Nationalism, Internationalism, and Peace," *Historian*, 36 (1974): 403-33.

²⁵ Jameson to Leland, Nov. 26, 1922, in *An Historian's World*, 275.

²⁶ Waldo G. Leland, "The International Congress of Historical Sciences, Held at Brussels," *AHR*, 28 (1922-23): 650-51.

he declared, concerned "the future of American scholarship, its dignity, its independence, its creative power."²⁷

Eager as Jameson was to see our history properly recognized at international congresses, he was principally determined to have the congresses produce some lasting benefit for historians and history and also bring historians together in friendly relations by working for a common purpose. The establishment of the International Committee of Historical Sciences in 1926, with a permanent bureau to provide continuity and leadership, was intended to develop projects with international support.²⁸ But only an International Bibliography of Historical Sciences received general support, and it has had a precarious existence. The statement made by Jameson still has some validity: "These congresses might have done more to promote the progress of historical science than merely to provide an opportunity for the reading of various papers and for social intercourse."²⁹

But what can historians do, scattered around the world as they are, following different approaches to history, living under different kinds of governments, with only a few able to attend the meetings held every five years? My own view is that we should encourage the ICHS to expand its activities between sessions on the basis of a few fundamental policies, such as the following.

First, access to archives should be liberalized. The VIth International Council on Archives in 1968 passed far-reaching resolutions on this subject. It urged that archival administrations of all countries review national regulations controlling access to documents and propose to appropriate authorities the removal of all unjustified restrictions. It recommended further that "the principle of equality of treatment between national and foreign scholars be recognized and applied everywhere."³⁰ Historians surely want to have as full access to sources as possible, and international pressure might be one of the best ways to achieve it. Should not historians, therefore, join with the archivists to work toward these desirable objectives? The ICHS would be expected to devise some procedure to handle complaints, perhaps in cooperation with the archivists. The experience of the AHA with the charges against the Franklin D. Roosevelt Library proves that this might be a heavy responsibility, but no announcement of principles governing access will be

²⁷ Haskins, "European History and American Scholarship," 215; see also Jameson to Henri Reverdin, May 24, 1923, in *An Historian's World*, 288.

²⁸ The literature on the history of the ICHS is scanty. In 1958 the bureau authorized Halvdan Koht and Waldo G. Leland to write a history. Koht prepared an eighteen-page account, *The Origin and Beginnings of the International Committee of Historical Sciences: Personal Reminiscences of Halvdan Koht* (Lausanne, 1962). Boyd Shafer kindly loaned me a copy of this rare work. Leland apparently never prepared anything on the subject except an earlier paper, "L'Organisation Internationale des Études Historiques," in *Historie et Historiens depuis Cinquante Ans: Méthodes, Organisation et Résultats du Travail Historique de 1876 à 1926*, 2 (Paris, 1928): 741-56.

²⁹ Jameson to Alexander S. Lappo-Danilevskii, Aug. 3, 1917, in *An Historian's World*, 214-15.

³⁰ For the complete text, see "Resolutions, Recommendations and Wishes of the VIth International Congress on Archives Held in Madrid, September 3-7, 1968," *Archivum*, 18 (1970): 213-15.

worth much unless there is some machinery for inquiry and redress of grievances.

Second, historians should be encouraged to study and teach in foreign lands. Jameson had ideas on this too. He proposed that British professors of history be invited to attend and participate in the annual meetings of the AHA, an invitation that might also involve their teaching in our universities. He once succeeded in getting support from the Carnegie Endowment for International Peace, and distinguished historians from a number of British universities attended the annual meeting in December 1924. Afterward a number of these visitors went to universities to meet with their colleagues and deliver lectures.³¹ Why should some similar arrangement not be developed for every session of the ICHS? How valuable it would be, for example, to our students and our faculties if fifty or more foreign historians who will attend the meeting in San Francisco could also teach for a quarter or semester before or after the meeting. The experiences of these historians on our campuses would also enlarge their understanding of life in the United States and the variety of historians to be found here. The matching of historians with appropriate institutions might require considerable managerial expertise, but it could be done.

Third, the teaching of history should receive sustained attention. Some sporadic attention has been given since World War I to the analysis of textbooks in order to eliminate gross prejudices and nationalistic bias, but the ICHS does not seem to have considered the improvement of history teaching as an essential part of its task. This is a curious fact. The modifying of national and other prejudices in the writing of textbooks should be one of the obvious and natural objectives of historians in their international organization. But attention to history teaching should not be limited to the ever-present problem of honesty and balance in textbooks. Is it not equally important for us to exchange ideas and experiences with our colleagues in other countries in order to improve the teaching of both our own national histories and the history of other cultures? This fundamental labor can probably best be undertaken at the primary- and secondary-school level, which means that we should ask the ICHS to develop some definite program for teachers in these grades to live and teach outside their own countries. Here indeed is a large and complicated enterprise in which the AHA is not yet fully equipped to participate, but our divisional committee for teaching should be very helpful in the future.

Besides these continuing activities for the improvement of accessibility to sources, travel for historians, and the teaching of history, the ICHS should re-examine the program and organization of its congresses. There must be better ways to foster understanding among historians than to mount expensive extravaganzas every five years.

³¹ Jameson to Elihu Root, July 19, 1923, in *An Historian's World*, 290-91.

The beginnings of the movement for the closer association of historians on an international basis were made by a small band of European and American historians in the early decades of this century, and in our present desire for improvement we must not forget or undervalue the pioneer efforts that made possible the present system of meetings every five years. Nor must we forget that most international movements develop very slowly and often involve disappointments and frustrations. But a larger and more solid structure for the international relations of historians is long overdue, and let us hope that at least the scaffolding for a new structure will have been constructed by the time the AHA completes its first century in 1984. When this comes to pass, all historians, no matter which tribe they belong to, will benefit.

IF JAMESON COULD VISIT us today he would doubtless be gratified to see how American studies, including history, are being increasingly cultivated in universities, institutes, and special associations in Britain, continental Europe, and elsewhere. The inadequacies that lasted into the late 1950s resulted from lack of funds, faculty resistance attributed to political opposition or doubt as to the academic validity of courses on the United States, and "the absence of young scholars with sufficient academic qualifications to merit appointment to university teaching posts in American studies."³² Thanks in part to the Fulbright program and foundation grants to the American Council of Learned Societies to encourage these studies overseas, the situation has changed radically in recent years.

Now the shoe is on the other foot. Japanese schoolteachers who studied at the University of Massachusetts, Amherst, last summer were shocked to find so little attention to Japanese history in our textbooks, just as scholars in Japan concerned with developing Latin American studies there deplore the ignorance in Latin America of Japanese culture.³³ Foreign historians, in the spirit of Jameson, may now be sensitive to what they view as our indifference to their increasingly important work on United States history. C. Vann Woodward deplored the parochialism of some American historians in these words: "The fault of Americans lies largely in their habit of looking

³² Gordon B. Turner, "A Decade of American Studies," *ACLS Newsletter*, 1970, no. 2, pp. 1-6.

³³ Gustavo Andrade, "Latin American Studies in Japan," *Latin American Research Review*, 8 (1973): 147-56. Dr. Andrade writes, somewhat in the spirit of Jameson, "This report has analyzed the state of studies on Latin America in the country which is one of the greatest economic powers of the world and which, according to the prediction of Herman Kahn, will be the country of the twenty-first century. And now let me ask, what does Latin America know about Japan? How many research centers and university departments are there which teach that Japan is no longer the land of cherry blossoms, because the fouled air of the great cities kills them, nor the land of Mount Fuji, because the smoke of the blast furnaces wipes its stylized figure from the landscape, nor the home of the geishas, because they prefer the easier road of the nightclubs? Where are the translations into Spanish of Nobel prizewinner Kawabata? If Latin Americans want the Japanese to understand the reality of Latin America, Latin Americans must also try to understand the reality of Japan" (pp. 155-56).

within for the significance of historical experience and assessing it narrowly according to preconceptions and legends of democracy, equality, and frontier-flavored determinants of exceptionalism." This myopia also explains why Americans have been so slow "to understand the significance of the influence they have exerted beyond their borders." David M. Potter gives substance to this charge, in examining the Civil War, for he concludes, "The significance of the Civil War for world history, and particularly for the history of nationalism, has been generally neglected by historians."³⁴

The quantity and quality of foreign contributions to United States historiography since the end of World War II will probably surprise many of us, and the AHA might well sponsor the preparation of an annotated and organized bibliography on the subject.³⁵ This bibliography would make clear that the increase of attention to our history abroad has not only been beneficial to the persons overseas whom Jameson worried about but would be equally useful to our own historians, for they would learn something about their own fields from foreign historians. A Dutch writer has stressed the difference between American and European scholarship: "European ideas that do not fit well into the American conception of self, that collide with the dominant official ethos of America, have long been soft-pedalled in American scholarly thought, while they prevail in European thinking."³⁶ There are differences, too, between American and European conceptions of social history, for different value systems result in different views. In the light of development abroad, must we not conclude that American history is too important to be left to American historians alone?

With the ever increasing attention the AHA is giving to teaching, why could we not sponsor, in various parts of the country and on a variety of topics, a continuing series of summer seminars and colloquia that would bring together historians from other parts of the world to discuss matters of mutual interest in the teaching and interpretation of American history? The foreign participants might spend an additional month or so visiting other colleagues or working in archival or library collections. Eventually American and foreign historians might work together on some aspect of

³⁴ C. Vann Woodward, "The Test of Comparison," in Woodward, ed., *The Comparative Approach to American History* (New York, 1968), 352; David M. Potter, "Civil War," in *ibid.*, 145. See also Peter Harnetty, "Cotton Exports and Indian Agriculture," *Economic History Review*, 2d ser., 24 (1971): 414-29.

³⁵ While gathering material for this paper, the following items came to my attention by chance: Inga Flots, *Colonel House in Paris* (Aarhus, 1972); A. N. J. den Hollander, ed., *Contagious Conflict: The Impact of American Dissent on American Life* (Leiden, 1973); "Theses on American Topics in Progress and Completed at British Universities," *Journal of American Studies* (published by Cambridge University Press), Apr. 1974, no. 1, pp. 131-51; Cristiano Camporesi, *Il marxismo teorico negli USA, 1900-1945* (Milan, 1973); Anna Katona, "Nineteenth-Century Hungarian Travelogues on the Pre-Civil-War U.S.," *Hungarian Studies in English*, 5 (1971): 35-52; "Nineteenth-Century Hungarian Travelogues on the Post-Civil-War U.S.," *ibid.*, 7 (1973): 51-94.

³⁶ A. N. J. den Hollander, "Cultural Diversity and the Mind of the Scholar," in Hollander, ed., *Diverging Parallels: A Comparison of American and European Thought and Action* (Leiden, 1971), 205.

our past. Would it not be refreshing to have a Brazilian scholar join with one of our historians to study the history of race relations in the United States?

Since Sahagún's fundamental work on Mexican Indians, studies of foreign cultures by scholars outside the cultures were sporadic until recently. Our institutions of higher education were parochial, for they recognized mainly the United States and Europe as proper subjects for scholarly inquiry and usually regarded other parts of the world as outposts on the periphery of civilization. The result, as Richard D. Lambert stated in his review of language and area programs, was that "generations of Americans educated before World War II were ill-equipped to live in the postwar world of newly independent nations asserting their rights to political sovereignty and to respect for their cultural identities."³⁷ Today the situation has radically changed, due to the energetic and far-sighted support for foreign area programs of the American Council of Learned Societies and the Social Science Research Council from the 1930s onward, with financial aid from foundations.³⁸ World War II prompted foreign language and cultural studies for strategic purposes. Since the end of the war development has been notable, for only thirty years ago "the American scholarly experts on many of the world's areas could have been assembled in a small room, and today all the world areas are represented by flourishing scholarly associations with memberships running in some cases into the thousands."³⁹ Throughout our colleges and universities one now finds a wide variety of well-trained area specialists, ready to enrich the educational offerings for their students with their hard-won knowledge of other cultures.

Many of these area specialists are historians, and now that few students are required to take courses in United States history or Western civilization, should not all history departments use their influence to encourage undergraduates to become acquainted, through a broad "civilization" course, with the history of another culture distinctly different from their own? World history will also have a place, particularly if presented with the imagination and expertise of a William H. McNeill, but the study of a single civilization has a special value all its own. Equally important would be the encouragement of graduate students in history to select one field from non-Western history for their general examinations. Enough good material now exists in English to make this a respectable and interesting possibility for all graduate students, and such broadening of their training

³⁷ Richard D. Lambert, "Language and Area Studies Review," *Items*, 27 (1973): 17.

³⁸ See Gordon B. Turner, "The Joint Committee on Slavic Studies, 1948-1971: A Summary View," *ACLS Newsletter*, 1972, no. 2, pp. 6-26; George E. Taylor, "The Joint Committee on Contemporary China, 1956-1969," *ibid.*, no. 4, pp. 1-16, and 1973, no. 1, pp. 11-32. For a list of the historians who enjoyed unusual opportunities to study foreign languages and cultures in these programs, see Dorothy Sunderland and Leslie Wendell, eds., *Directory: Foreign Area Fellows, 1952-1972, of the Joint Committee on the Foreign Area Program of the Social Science Research Council, 1962-1972* (3d ed.; New York, 1973).

³⁹ Lambert, "Language and Area Studies Review," 17.

would also enlarge their possibilities as teachers. This training would, in addition, increase their ability to treat topics of comparative history. The stimulating contribution of Carl N. Degler on race relations in Brazil and the United States indicates what we may expect when practitioners in one field enter another.⁴⁰

Fifty years ago Haskins felt that one of the important obstacles to American research on European history was the deficiencies of our libraries. Today it is possible to pursue meaningful research on most areas of the world without leaving the United States, and in many fields our library resources are unsurpassed. A large volume would be required to do justice to this subject. Let these illustrations indicate the depth and range of the documentation available on foreign areas: in the period 1962–67, the Library of Congress offices abroad obtained 7.5 million publications from Ceylon, India, Indonesia, Nepal, the United Arab Republic, and Yugoslavia. Through this program forty other research libraries received sets of foreign-language publications, and 310 libraries received English-language sets.⁴¹ The April 1965 issue of the Library of Congress's *Monthly Index of Russian Accessions* contained 487 pages of triple-column pages in small type.

Another way in which American historians might improve their world view would be to hold an annual meeting in Mexico City. We have met twice in Toronto: why not follow the example of the American Association for the Advancement of Science and other American professional associations by trying out the excellent facilities in Mexico City? It would be worth the trip alone to visit the Anthropological Museum there, a remarkable testimony to the Indian cultures whose study Sahagún initiated.

One possible danger must be mentioned. As our students and professors become more acquainted with the history and conditions of other tribes, will we become more sensitive to injustices committed abroad, especially to historians, and in consequence will we attempt to influence foreign nations in ways we consider desirable? Spaniards studied Indians largely as an aid to Christianizing them. Will the AHA look upon governments and historians that do not follow our ways as laggards in civilization who must be exhorted by formal resolution and even condemnation to follow our leadership on such explosive matters as civil rights and free speech?

These are gut issues on which honest historians differ. Thus far the Soviet Union's treatment of its dissident intellectuals has received most attention, but if relations between the People's Republic of China and the United States continue to increase, there will be other problems to confront. Although ethnocentrism can be found in many places—and there may be even today some Americans who would agree with Jameson that what the world needs is a large dose of American history to save it—China

⁴⁰ Carl N. Degler, *Neither Black nor White: Slavery in Race Relations in Brazil and the United States* (New York, 1971).

⁴¹ Dagmar Horna Perman, ed., *Bibliography and the Historian* (Santa Barbara, 1968), 59.

has one of the most completely closed civilizations ever developed in the world. Until the 1840s Chinese governmental and educational elites saw little need to study foreign languages or cultures, for all non-Chinese were considered barbarians. Those few who did study these subjects were dubbed "barbarian tamers" and tolerated because they performed an "odious and distasteful job, like sewer-inspectors," an attitude that lasted in some quarters well into the nineteenth century.⁴² Today there is a different orthodoxy in China, according to which Maoist values are enshrined as the ultimate repository of truth. Apparently we will see in China a conscious and continuous ideological orientation of historical scholarship, as has been the case for some time in other countries, which will make dissidence dangerous and unlikely.⁴³

Americans living in a pluralistic society where there is legal emphasis on individual rights and free speech look upon such cultures as subject to thought control, whether in China, Saudi Arabia, the Soviet Union, or elsewhere. Can historians from widely varying cultures find common ground to stand on? If one may judge from our experience in discussing Latin American history with Soviet scholars, the possibilities of a fruitful exchange of views must not be exaggerated.⁴⁴ Thus far these exchanges have been limited because of financial, linguistic, and political reasons, but it is likely that the coming generation will see a more wide-ranging and intense debate than ever before as historians discuss Latin America from the standpoint of their own tribes.

A final problem must be mentioned: the function of "tribal history." Do all nations—including the United States—need parochialism, naïveté, and myths to bind together their people? Does everyone need to cultivate

⁴² John King Fairbank, *Trade and Diplomacy on the China Coast: The Opening of the Treaty Ports* (Stanford, 1964), 176. An important recent study by Donald W. Treadgold deals in detail with both Russian and Chinese responses to the West. *The West in Russia and China* (Cambridge, 1973). Other valuable studies of this complicated topic are Paul A. Cohen, *China and Christianity: The Missionary Movement and the Growth of Chinese Antiforeignism, 1860-1870* (Cambridge, Mass., 1963), and Joseph R. Levenson, *European Expansion and the Counter-Example of Asia* (Englewood Cliffs, 1962). For an example of how cautiously one scholar worked to help his countrymen comprehend the nature of the outside world, see Fred W. Drake's account of the history-geography of Hsu Chi-yu (1795-1873). "A Mid-Nineteenth-Century Discovery of the Non-Chinese World," *Modern Asian Studies*, 6 (1972): 205-24. For a sophisticated and forthright statement on the role American historians should play in the study of Chinese history, see John K. Fairbank, "Assignment for the '70's," *AHR*, 74 (1968-69): 861-79.

⁴³ Herbert A. Simon, "Mao's China in 1972," *Items*, 27 (1973): 1-4. On ideological aspects of history writing in the German Democratic Republic and in Poland, see the review by George G. Iggers, *Journal of Modern History*, 44 (1972): 149-52.

⁴⁴ I. R. Lavretskii, "A Survey of the Hispanic American Historical Review, 1956-1958," *Hispanic American Historical Review*, 40 (1960): 340-60. This article originally appeared in *Voprosy istorii*, and the survey concluded "that the official Latin Americanists of the U.S. falsify and distort the historical truth in order to benefit imperialism" (p. 360). See also Russell H. Bartley, "On Scholarly Dialogue: The Case of U.S. and Soviet Latin Americanists," *Latin American Research Review*, 5 (1970): 59-62. This is an introduction to the article by M. S. Al'perovich, "Soviet Historiography of the Latin American Countries," *ibid.*, 63-70. For a Mexican perspective, see Juan A. Ortega Medina, *Historia soviética iberoamericanista* (Mexico City, 1961).

self-sustaining, self-satisfying, and supportive notions about the virtues and unique qualities of the tribe he belongs to? Perhaps so, and if the tribal history can be kept within decent bounds by the perspectives of historians inside and outside the tribe, it may serve a useful purpose. It must be recognized, too, that not all members of a tribe accept the dominant interpretation of its history and that divergent opinions within a tribe affect the views of historians outside. A century ago Japanese educational leaders embraced the American dogma of hard work and individualism—"Boys, be ambitious" was the watchword transmitted to Japanese youth by William Clark of Amherst.⁴⁵ Japanese Americanologists such as Yasaki Takagi, who introduced a course on the United States at Tokyo University in 1918, were convinced that America was basically "a good country of good people." The generation after 1945 was not so sure, and it aimed at viewing America objectively and dispassionately. A new school, now gathering influence, "contends that earlier American studies in Japan, following the example of American scholars themselves, have ignored the problems of America's minorities—the blacks, Indians, and immigrant groups—and is in need of fundamental reform."⁴⁶ Today Japanese textbooks no longer reflect the simplistic image that summed up for previous generations the message of America: "Boys, be ambitious."

If myths are useful when held by members of a tribe concerning its own history, they are less innocent and less justifiable when they are invoked to explain another culture. For example, during the Vietnam War the United States stressed its opposition to Communist North Vietnam as a totalitarian dictatorship similar to those found elsewhere. The difficulty with this argument Frances FitzGerald has made clear: "The non-Communist Vietnamese leaders believed in intellectual freedom no more than the Communists. . . . Intellectual freedom, of course, implies intellectual diversity."⁴⁷ Is not one of the important reasons for paying attention to the history of other peoples to make sure that our understanding of their culture is not based on untenable myths?

However we may answer these questions, I believe that historians in this country now face a watershed, just as did that small group of teachers and writers who founded the AHA in 1884, who aimed to raise the teaching of history to a higher level because they were convinced that the local and

⁴⁵ Quoted by the staff of the Asahi Shimbun in *The Pacific Rivals: A Japanese View of Japanese-American Relations* (New York, 1971), 363. It is encouraging to see that some Japanese historians, such as Masuda Yoshio, are also calling, as Lothar G. Knauth has said, for "less parochialism among Japanese historians and the removal of barriers between the Japanese historians of Japan and those of foreign countries. Only in this manner, he insists, can Japan come to grips with the problem of its place in world history and overcome her relative alienation in Asia and in the world." "Pacific Confrontation: Japan Encounters the Spanish Overseas Empire, 1542-1639" (Ph.D. dissertation, Harvard University, 1970), 530-31.

⁴⁶ Staff of the Asahi Shimbun, *Pacific Rivals*, 355-57.

⁴⁷ Frances FitzGerald, *Fire in the Lake: The Vietnamese and the Americans in Vietnam* (New York, 1973), 22.

state spirit should give way to a larger, national view.⁴⁸ Our problem today is to find ways of strengthening all international aspects of history teaching and writing in the United States. I am convinced—and this may be an expression of my own ethnocentrism—that no nation today has a better opportunity than our own to attempt to study other cultures without necessarily losing the necessary life-giving and life-sustaining connection with our own national roots. A minority group like the Scots or the Catalans may have some justification for giving almost exclusive attention to their own history, lest they disappear as a distinct culture. But surely the situation is different in the United States, with its many different strains of cultures, with its economic and political power, and with the need to overcome or at least diminish and channel in other directions the force of what might be called its missionary zeal.

My hope is that in the great enterprise, whose dimensions I have barely sketched, organized American historians will have an important and even indispensable part. Today the AHA has more projects, more problems, and a larger budget than ever before. Among our 17,000 members is to be found an astonishing diversity of historical interpretations, life-styles, linguistic skills, and, yes, pizzazz. Surely this remarkable aggregation of human beings will be able to influence the study and teaching of history in international as well as national ways and to strengthen the already solid beginnings made here to study seriously the history of other nations and other peoples, while continuing to help Americans understand their present and future by providing an honest and informed picture of the past. When this day arrives we shall be achieving what Jameson hoped for: recognition of the fact that the history of the modern world cannot be fully understood unless foreign historians pay more attention to our history and recognition that United States history cannot be fully comprehended if isolated from world history.

Americans will then be ready for an even more difficult step, the initiation of fundamental revisions in their own views of the world, man, and the future, which began in the century of the great discoveries and for which Bernardino de Sahagún showed the way by his studies of Aztec culture. If American historians are fully aware of their opportunities and responsibilities in the world today, they can exert a powerful influence by their teaching and research to the end that we are able to appreciate the history of other peoples without losing allegiance to our own. By studying the history of their own tribes and other tribes as well, historians should be in the forefront of all those who would seek to understand the common elements in all cultures.

⁴⁸ David D. Van Tassel and James A. Tinsley, "Historical Organizations as Aids to History," in William B. Hesseltine and Donald R. McNeil, eds., *Essays in Memory of Herbert A. Kellar* (Madison, 1958), 62.

Immigrants from Islam: The Crusaders' Use of Muslims As Settlers in Thirteenth-Century Spain

ROBERT I. BURNS, S.J.

DURING THE SECOND QUARTER of the thirteenth century, King James the Conqueror of Arago-Catalonia led an international body of crusaders in a series of stubborn campaigns down the eastern coast of Spain. The crusade won an extensive kingdom of Valencia, roughly the size and shape of the crusader Holy Land, whose coastal cities bustled with commerce and whose hinterland comprised an agricultural cornucopia. Valencia spelled riches and rents for the happy crusaders as long as skilled manpower remained available to maintain the complex organization of its irrigated *huertas* and busy ports. Having expanded too far and too fast in an age of easy opportunity, however, King James found himself unable to induce more than a trickle of Christian settlers to come south. Where he needed a minimum of 100,000 settlers to guarantee basic military security, James confessed thirty years later, he achieved an eventual total of under 30,000. Worse, the crusade and subsequent rebellions caused a hemorrhage of Muslims away from towns and countryside toward the distant havens of Islamic Granada and North Africa.¹

Though the crusaders, from the king down, had deployed fierce rhetoric about expelling all Muslims beyond the boundaries of the new kingdom, they had in fact gone to extraordinary lengths to retain in place every

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¹ Background on the crusade is in Robert I. Burns, S.J., *The Crusader Kingdom of Valencia: Reconstruction on a Thirteenth-Century Frontier* (Cambridge, Mass., 1967). On Mudejarism see his *Islam under the Crusaders: Colonial Survival in the Thirteenth-Century Kingdom of Valencia* (Princeton, 1974) and its sequel, *Medieval Colonialism: Postcrusade Exploitation of Islamic Valencia* (Princeton, 1975). A further work, "The Crusader-Muslim Predicament: Colonial Confrontation in the Conquered Kingdom of Valencia," is currently in preparation. Each book has ample bibliographical orientation. Other monographs and articles pertinent to the Muslim-Christian situation in Valencia include Burns's "Spanish Islam in Transition: Acculturative Survival and Its Price in the Christian Kingdom of Valencia," in Speros Vryonis, Jr., ed., *Islam and Cultural Change in the Middle Ages* (Berkeley, 1974); "Social Riots on the Christian-Moslem Frontier: Thirteenth-Century Valencia," *AHR*, 66 (1960-61): 378-400; "Christian-Islamic Confrontation in the West: The Thirteenth-Century Dream of Conversion," *AHR*, 76 (1971): 1386-1434; "Le royaume chrétien de Valence et ses vassaux musulmans," *Annales: Économies, Sociétés, Civilisations*, 28 (1973): 199-225; and his series of articles in *Speculum* from 1960 to 1971.

Islamic community and Muslim farmer or craftsman. Such exceptions as the expulsions at Valencia city and Burriana or the extrusion of aristocratic rebel groups prove upon close examination to support this general rule. Even where the surrender treaties allowed Muslim emigration to Islamic lands the crusaders sometimes sought to evade the permission. The banners of Muslim castellans still ruling from many strongholds and the call of muezzins from the ubiquitous minarets served to underline this deliberate establishment of Islam as a component of the new kingdom. Thus the crusade that began in paradox—when a Christian king joined forces with the last Almohad ruler in Spain to suppress an intra-Islamic revolt—ended in paradox: vowed before Christendom to expel the enemy, the king found himself as disturbed over the falling level of Muslim manpower as over the equally ominous predominance of Muslims. The apparent conflict between King James's determination to preserve his Muslim *aljamas* intact and his boasts of having expelled them has stimulated historians to variously implausible exegeses and resolutions. My own interpretation, offered elsewhere at length, points to minimal and reluctant expulsion, to a steady policy of retaining the Muslim communities in both cities and countryside and on Crown as on baronial lands, and simultaneously to serious losses on account of voluntary departure over the decades.²

In this context the fairly startling sight of crusaders enthusiastically importing Muslim settlers takes on more meaning. Following traditional precedent all classes of landlord, ecclesiastical and lay, could prosper in direct proportion to their ability to attract onto their lands fresh supplies of Muslims. This expedient muddled the clear image of crusade, as contemporary protest proves, but the prospect of revenues overrode all objection. The Crown desperately required such revenues, to continue its transformation from suzerain to sovereign status and to raise Arago-Catalonia higher in the ranks of the maritime powers. The barons, caught in a future shock that threatened to displace them like dinosaurs, scrambled even more greedily for rents and investments. Churchmen, including the military orders, had an omnivorous appetite for funds to meet increasing challenges. And the town or rural patriciate were cannily working their several ways up a golden ladder of prosperity and status. All participated in the game. It was a game disdained by the Holy Land crusaders, though parallels might be found elsewhere in Christendom.³

² Burns, "Expelling the Muslims from Thirteenth-Century Spain: A Revisionist Interpretation," paper delivered at the seventh annual meeting of the Middle East Studies Association of North America in Milwaukee, November 9, 1973.

³ Bela IV of Hungary (1235-70), unable to gain a European crusade to defend the east against the Mongols, intensified a policy of his Arpad dynasty by attracting French, Italian, German, and other immigrants; he especially brought in blocs of pagans, such as the detested Cumans. In the Holy Land, despite great need, Christian repopulation was the norm. See Claude Cahen, "Le régime rural syrien au temps de la domination franque," *Bulletin de la faculté des lettres de Strasbourg*, 29 (1951): 286-310; see also Joshua Prawer, "Colonization Activities in the Latin Kingdom of Jerusalem," *Revue belge de philologie et d'histoire*, 29 (1951): 1063-1118, including the economic and political crisis threatened by large-scale Muslim emigration (pp. 1083-85); and Prawer, *The Crusaders' Kingdom: European Colonialism in the Middle Ages* (New York, 1972), 82-84.

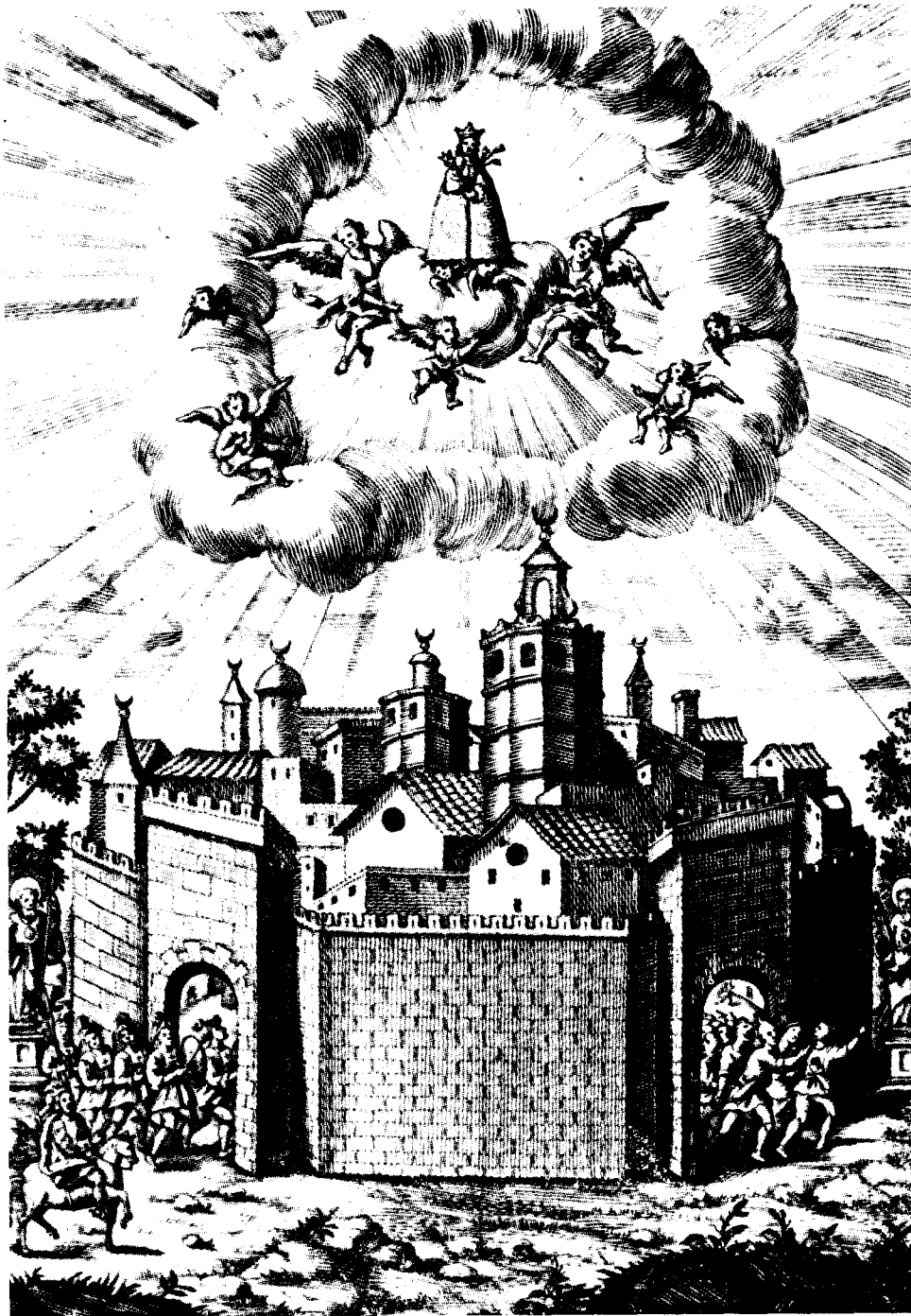


Fig. 1. A display from the celebration in 1740 of the expulsion of the Muslims from Valencia. The original, erected at St. Andrew's Church in Valencia, was a model of the Islamic city (basically a stylized eighteenth-century Valencia with crescents replacing crosses). Machinery kept the crusaders "in continual motion" entering at left and the Muslims going into exile at right, symbolizing the replacement throughout the kingdom. The Virgin of Victories, in the air, was believed to be a copy of the image King James honored in his tent during the siege. The flanking statues of Peter and Andrew each had a little relic of the respective saint in the niche inserted within its breast. Flowers and candles abounded around the model. The original picture, drawn from the huge model, is one of a number the artist drew from the many such displays in 1740. From Joseph Vicente Ortí Mayor, *Fiestas centenarias con que la insigne, noble, leal, y coronada ciudad de Valencia celebró en el día 9 de Octubre de 1738 la quinta centuria de su cristiana conquista* (Valencia, 1740), facing page 125. A copy is in the Biblioteca Municipal of Valencia.

Though individual scholars have caught a glimpse of the immigration phenomenon at the periphery of one or other field of special interest, it has never been isolated for study.⁴ Consequently it has been easy to view the phenomenon as a rare expedient applied at random or as perhaps the replacement of expelled rebels at an inconsiderable number of places. But the total process, viewed whole, reveals a different scene. This merits direct confrontation, to establish its reality and universality as well as to open discussion as to its pattern and cumulative impact. To explore it fully one must combine ecclesiastical and secular documentation of varied genres; voices from the Vatican Archives must mingle with those from the manuscript collections at the king's Barcelona palace, and crusader treaties with Templar or cathedral instruments.

LONG BEFORE VALENCIA'S CONQUEST Christians had imported Muslims to augment a local working force or to extend an agricultural frontier. "Barons and other knights, as well as townsmen" under Alfonso II a century before so widely encouraged Muslim immigration, dividing their "estates and properties among Saracen owner-renters [*exarici*]," that "dissension was stirred up" with the Church over exemption of such tracts from Christian tithe. This quarrel, settled by royal decree in 1167, involved only land worked by Muslims under contracts drawn after the conquest—land never previously held by Mudejars and not owned by current Mudejar tenants.⁵ A like dispute caused Innocent III to lodge a protest in 1199 with the bishop of Ávila in neighboring Castile.⁶ The thirteenth-century Crown of Arago-Catalonia continued to allow the practice, as when King James in 1262 chartered the monastery of Piedra, near Calatayud in old Aragon, with a perpetual privilege of bringing in settlements of Christians and Muslims for three colonizing projects.⁷ Such shifts of population may have amounted only to internal immigration or movement from one point to another within the federated kingdoms of Arago-Catalonia. Surplus population at the point of origin, brighter opportunity at the point of arrival, or some

⁴ Thus Elena Lourie, in the course of her investigation into "Free Moslems in the Balearics under Christian Rule in the Thirteenth Century," *Speculum*, 45 (1970): 624-49, speaks in passing of a colonizing movement of this kind on Majorca (p. 625), Ibiza (p. 631), and ambiguously Minorca (p. 632), and of two papal protests (p. 628).

⁵ King Alfonso II to the Church of Tarazona, Aug. 3, 1167, in Francisco Fernández y González, *Estado social y político de los mudejares de Castilla, considerados en sí mismos y respecto de la civilización española* (Madrid, 1866), app., doc. 8. The context makes it clear that the argument was not merely about a landlord's personal income derived from Muslim tenants but from the Muslim-held farm itself when the Muslim had been introduced after Christian ownership of the farm was an established fact.

⁶ Pope Innocent III to the bishop of Ávila, May 21, 1199, Archivio Segreto Vaticano (hereafter Arch. Vat.), Rome, Reg. Vat. 4 (Innocent III), fol. 160v; also published in Demetrio Mansilla Reoyo, ed., *La documentación pontificia hasta Inocencio III (965-1216)* (Rome, 1955), doc. 193: "qui nolunt ecclesiis, sicut olim christiani solebant, freti potentia et favore illorum a quibus illis excolenda traduntur, decimas exhibere."

⁷ King James I to Piedra monastery, Dec. 20, 1262; see Joaquín Miret y Sans, *Itinerari de Jaume I "el Conqueridor"* (Barcelona, 1918), 332.

adventitious factor such as the king's desire to thin out the Muslims of frontier zones may have been elements in a given transfer.

As a prelude to the Valencian crusade King James had been careful to conquer the flanking Islamic principalities on the Balearic Islands. His policies on this lesser stage were a dress rehearsal for management of the later Valencian kingdom. Thus colonization by transfer of Muslims was proceeding apace in the islands on the eve of the Valencian crusade. In 1231, the year before the crusaders started south, the Templars received a Crown charter to "settle and house thirty households of Saracens at whatever place you wish in your share" of Majorca. To attract the Muslims, King James exempted them and all their descendants from regalian taxes, placing them under his protection.⁸ This was no isolated instance. So assiduously did the Templars and Hospitallers colonize the Balearics with Muslims during the subsequent decade that Rome intervened. In 1240 Pope Gregory IX sent a brief rebuking the practice to Prince Peter of Portugal as Majorca's lord and to all landowners including the two military orders. Gregory urged that they "by no means permit the said islands to be peopled by Saracens" but only by Christians.⁹ Necessity overrode such appeals. Eight years later another pope, Innocent IV, found it necessary to launch a similar public directive, addressed this time directly to King James.¹⁰

Mudejarism in Valencia, though rooted in such traditional policy, had to be applied on a scale so extensive as to transmute the experience qualitatively. The importation or resettling of Muslims and the multiplication of Islamic *aljamas* consequently assumed unprecedented proportions. Almost immediately upon the fall of Valencia city, as the first extensive conquests underwent reorganization, the metropolitan archbishop of Tarragona considered it among his more urgent duties to excommunicate landlords importing Muslim settlers. This information has survived only in roundabout fashion. As part of maneuvers preceding an ecclesiastical trial for metropolitan control of the Valencia diocese, Toledo and Tarragona laid legal foundations by exercising such important acts of jurisdiction as giving indulgences and publishing excommunications. At the trial John Gonsalvo, from the household of the baron Peter Ferdinand, testified how "he saw

⁸ King James I to the Templars of Majorca, July 8, 1231, excerpted in *ibid.*, 94: "populare et casare." Lourie believes these Muslims "had been imported from the Templar properties on the peninsula." "Free Moslems in the Balearics," 625-26. A. J. Forey finds her evidence for this "by no means conclusive." *The Templars in the Corona de Aragón* (London, 1973), 250n. Both authors miss the essential circumstance that the document refers to no specific Muslims, whatever their origins, leaving the Templars free to attract them at large; "quos habeatis proprios et francos" merely exempts from regalian control and taxes.

⁹ Pope Gregory IX to Prince Peter of Portugal, Jan. 25, 1240, Arch. Vat., Reg. Vat. 19 (Gregory IX), fol. 143v, letter 226: "dictas insulas ab eisdem sarracenis populari minime permittatis."

¹⁰ Pope Innocent IV to King James, Feb. 28, 1248, Archivo de la Catedral de Palma de Mallorca, no. 13,451; also published in part by Jaime [with pseudoauthor Joaquín] Villanueva, *Viage literario a las iglesias de España* (Madrid, 1803-52), 21: 131.

him [the Tarragona archbishop] excommunicating all those who bring in Saracens to settle their estates." That this incident did not merely anticipate future abuses becomes apparent from the context; in the face of a captious rival, the archbishop had selected solid actions useful in a court of law to establish his ownership.¹¹

The practice of importing Muslims also comes into view through the disapproval expressed by the first bishop of Valencia as early as 1240 in a tithe agreement with the Crown. "Saracens colonized on lands freely acquired, though we do not approve their use in populating and indeed rebuke it, are to give full tithe on everything." The text distinguishes them from Muslims who had surrendered during the past decade and kept their lands. The bishop, resigned to a continuing program of such immigration, provided for lands both "settled and to be settled from now on."¹²

The decade after the fall of Valencia city saw the importation of Muslims swell until the situation got out of hand. The Tarragona metropolitan, with his suffragan bishops and King James, considered it grave enough to warrant a united appeal to Rome. In about 1245 they requested from Pope Innocent IV "a general sentence of excommunication against all in the kingdom of Valencia who, on places the Christians had taken from Saracens by armed force . . . , colonize Saracens or bring them back."¹³ Rome was slow in complying. Valencia meanwhile experienced a general revolt, with the subsequent expulsion of a number of Muslims. Not only the barons but religious orders and the king himself now joined the movement of introducing aliens in quantity. The Valencia bishop was particularly unhappy over this development. The newcomers got lands ideal for Christian settlement, further depriving the diocese of future tithes; the circumstance that in Valencia tithes went to diocesan authorities rather than to parishes surely sharpened his anguish. In answer to renewed complaint, this time from the bishop, Pope Innocent finally in 1251 chose to answer the king's plea for an excommunication. Embarrassingly enough, owing to these changed circumstances, the excommunication might now fall on King James himself.

The pope included a sharp reprimand. "Certain barons and exempt religious, and also others who . . . hold castles and towns as well as other possessions, have brought in and are bringing in Saracens to settle at many places of the same realm, contrary to the vow made by the said king and to the aforesaid [episcopal] excommunication, to the peril of their souls and the serious detriment of the church in Valencia—the king himself

¹¹ "Ordinatio ecclesiae valentinae," trial record edited in José Sanchis y Sivera, *La diócesis valentina, nuevos estudios históricos* (Valencia, 1921), 360.

¹² Tithe agreement between the bishop of Valencia and King James I, 1240, Archivo de la Catedral de Valencia (hereafter Arch. Cat. Val.), parchment 1,304.

¹³ The date of the original letter is unknown, but information about it is contained in the reply. Pope Innocent IV to the bishop of Valencia, Dec. 23, 1251, Arch. Vat., Reg. Vat. 22 (Innocent IV), fol. 133v, letter 146: "popularent sarracenos vel reducerent."

doing the very same thing in some places." Innocent recognized that circumstances were abnormal. Instead of pressing for cessation he returned the situation into the hands of Valencia's bishop, stating his anxiety for the souls of the king and other malefactors and his concern over destruction of the diocese's financial underpinnings wrought by such colonization. For the good of souls and the new kingdom Innocent directed the bishop to act as seemed best "about commuting the king's vow, expelling or keeping the Saracens, relaxing or strengthening the aforesaid excommunication, as also about the other matters above." He adjoined the condition, "provided that by settling of Saracens the Church herself is in no way defrauded of the tithe and her other rights in any place."¹⁴

Pope Innocent's decision was prudent. Given the system of Mudejarism in effect, which established all Islamic communities of Valencia as an enclave society in many ways autonomous, it must have seemed logical to expand it wherever necessary or reasonably useful. It must also have seemed inevitable. If either party—royal and burgher, or baron and knight—propped up its failing finances in this way or realized any economic advance in this era of social struggle the other party would certainly follow suit. Both parties could sidestep ecclesiastical penalties by pleading such excusing causes as the many Catalan graduates of Bologna's law faculty might hastily devise. After all, Majorcan neighbors had entered upon forbidden trade with the Muslim enemy and had recently won papal permission to continue it by their plea that "a new settlement" like Majorca could not hope for increased Christian immigration without this commerce.¹⁵

By 1283, when a revision of the Valencian kingdom's law code restored baronial privileges that had recently suffered erosion, King Peter took care to insert a statute reaffirming that "any person of the city and realm can send Saracen workers to work his estates for a period of time or in perpetuity." The same sentence distinguished newcomers from Muslims "who already reside on the same estates." Stabilizing four decades of evolution, this statute may serve to introduce a roll call of sample cases that illustrate and clarify this fascinating story.¹⁶

SUFFICIENT EVIDENCE OF A GENERAL NATURE remains to establish the movement's existence and scope. Particular documentation, for this as for most other facets of thirteenth-century Valencia, survives only in random samples.

¹⁴ *Ibid.*: "quidam tam barones quam religiosi exempti ac alii etiam qui castra et villas necnon et possessiones alias possident sarracenos in plurima loca eiusdem regni, contra votum dicti regis emissum et predictam sententiam, in suarum animarum periculum et grave preiudicium ecclesie Valentine ad populandum induxerint et inducant eodem rege faciente in locis aliquibus illud idem."

¹⁵ [Pope Innocent IV] to the bishop of Majorca [confirming the permission of Pope Gregory IX], Mar. 21, 1248, *ibid.*, Reg. Vat. 21 (Innocent IV), fol. 516v, letter 682.

¹⁶ Statute in the Valencian law code, *Fori regni Valentiae*, ed. F. J. Pastor and P. J. de Capdevila (Monzón, 1547–48), bk. 8, rubric 8, no. 28.



Fig. 2. A thirteenth-century Mudejaresque plate from the celebrated Moorish ceramics center of Paterna, in the Valencian *huerta* just upriver from Valencia city. The figure represents the lord of Paterna, bearing his Aragonese Artal de Luna coat of arms as differenced for Paterna. This is the kind of local lord then settling Muslims on his estates. The Mudejaresque production recalls the symbiosis of Christian lord and Islamic subject. The source, ceramics, suggests a neglected historical source and a new methodological approach. Less than half life-size here, the plate has a characteristically greenish cast, interworked with brown. The original is in the Museo Nacional de Cerámica "González Martí" de Valencia.

They are frequent and clear enough, however, to fill in those broader outlines. The fairly well-documented military orders, for example, had many Muslim tenants throughout their estates. They were also accustomed to importing supplementary workers, particularly slaves or prisoners of war. At the surrender of Chivert in 1234 the Knights Templar took care to lure back families who had fled abroad during the crusade, inviting "any Saracen of Chivert who has taken up residence in a land of the Saracens" to return within a year and recover his "houses and estates and other possessions" without loss. If the owner wished to remain in international exile or died before being able to return, his heirs received all properties, the complexities of Islamic inheritance law prevailing. A similar proviso appears in the sur-



Fig. 3. A thirteenth-century Mudejaresque plate from Paterna. Christian settlers gradually infiltrated the Muslim ranks in this Hispano-Arab ceramics center and adapted its art. Although Muslims long dominated here, it is possible that this plate is by a Christian. Its conjunction of two characteristic zoomorphic themes, the peacock and the fish, is understood by some scholars to symbolize the coexistence in Valencia of Islam and Christendom. The plate is about a foot in diameter and is dominantly green in color, with some brown trim. The original is in the Museo Nacional de Cerámica "González Martí" de Valencia.

render charter by the Crown to the Muslims of Eslida, and by implication in the charter of Uxó. On balance it seems probable that this was a normal element in any surrender or amnesty.¹⁷

¹⁷ Joaquín Miret y Sans, *Les cases de templers y hospitalers en Catalunya, aplech de noves y documents històrics* (Barcelona, 1910), 145, and his "Inventaris de les cases del temple de la corona d'Aragó en 1289," *Boletín de la real academia de buenas letras de Barcelona*, 6 (1911): 72: "quantos sarracenos habetis in honoribus vestris . . . aut illos que de aliis partibus pro amore Dei adduxeritis vel de Ispania quos Deus ibi vobis dederit"; "Spain" meant the Islamic parts; carta puebla de Chivert, Apr. 28, 1234, no. 76 in "Colección de cartas pueblas," *Boletín de la sociedad castellonense de cultura*, 24 (1948): 226-30; carta puebla de Eslida, May 29, 1242, in Fernández y González, *Estado social y político de los mudejares de Castilla*, app., doc. 17.

After the conquest and first settlement of the Valencian kingdom had rendered secure the southern border regions of the old kingdom or province of Aragon proper, the Templars introduced Muslim settlers there, at Villastar near the central Valencian frontier. Previously unsuccessful in attracting sufficient Christians, the order set up this *aljama* of thirty Muslim families in 1267; besides the newcomers, probably from nearby Valencia, the only other residents were four Christian farmers and the Templar garrison at the local fort. The number of Muslims, recurring in other documents, suggests that something over a hundred souls made for a stable and varied rural community. The Hospitallers, an order experienced in settlement projects, similarly imported Valencian Muslims to Aldea, just over the northern border of the conquered kingdom: "Saracens were brought up from Silla with our permission and settled by the master and brothers of the Hospital in the aforesaid place." The year was 1258, a particularly active time for rearranging population balance because of the recent revolt. In the same year the religious Knights of Calatrava colonized their Burriana estates with Muslims. During an earlier flurry of expulsion and settlement, the Hospitallers in 1248 had arranged to settle a hundred Muslim families at the Albufera lagoon in order to supply boatmen for their fleet of thirty fishing ships.¹⁸

An early bishop of Valencia, more flexible than his excommunicating predecessor, brought in Muslim settlement for diocesan lands at Garg; King Peter approved the project in 1280, adverting to two classes of colonizers. "Know that we . . . have granted the venerable bishop of Valencia that all the Saracens native to Garg may come, if they wish, to settle in that castle and to dwell there; likewise also those [alien] Saracens . . . who came to settle wherever we would want them, may come if they wish to settle, as is said above, at the above-mentioned castle of Garg." In support of the program a decree went out next day ordering Crown officials to respect this charter.¹⁹ By anticipation, King James had pregranted Tales to the

The Uxó charter excluded fugitives who had recently besieged the king there: "e tots aquells que isqueren de la vall de Uxo, e no[s] forem assetjats ab ells en lo dit castell." Aug. 1250, in *ibid.*, doc. 23. Both its position and its wording oppose the interpretation of Miguel Gual Camarena that it was a general prescription. "Mudéjares valencianos, aportaciones para su estudio," *Saitabi*, 7 (1949): 172. The Játiva charter made provision "quod si aliquis serracenus venerit ad populandum in ravallum predictum" and wished to leave after one or several years, he could do so. Carta puebla de Játiva [Jan. 23], 1251 or 1252, in Fernandez y González, *Estado social y político de los mudejares de Castilla*, app., doc. 24.

¹⁸ Carta puebla de Villastar, July 15, 1267, in Forey, *Templars in the Corona de Aragón*, app., doc. 24, and see pp. 218, 221; King James I, charter of protection to Muslim settlers from Silla, Feb. 19, 1258, in *Colección diplomática de Jaime I, el Conquistador*, ed. Ambrosio Huici Miranda (Valencia, 1916-22), 3: 187, doc. 766; King James I to the Knights of Calatrava, Mar. 16, 1258, in Ramón de María, ed., *El "Repartiment" de Burriana y Villarreal* (Valencia, 1935), 71-72; King James I to Hospitallers, Feb. 5, 1248, in Francisco Diago, *Anales del reyno de Valencia* (Valencia, 1613), 1: 346, a transcription from the archives of Valencia kingdom of a document now lost. For background on the Hospitallers' skill and role in bringing in settlement as a general policy, see Burns, *Crusader Valencia*, 1: 189-90.

¹⁹ King Peter III, charter in favor of the bishop of Valencia, May 10, 1280, Archivo de la Corona de Aragón (hereafter Arch. Crown), Barcelona, Peter III, Reg. Canc. 44, fol. 182. The decree of May 11, 1280, is in *ibid.*

knight Peter of Castellnou as early as 1225, to populate with thirty Muslims. After conquest and pacification of the area, Castellnou in 1260 offered the Muslims a liberal constitution.²⁰ The king also licensed Bernard of Juneda in 1258 to settle Muslims in this region:

We and ours grant permission and power to you Bernard of Juneda, resident of Onda, that you may settle Saracen men and women, whomever and in whatever number you desire, on the lands you hold in the settlements of Artesa, Tales, and Cavallera, which are in the district of Onda; which Saracen men and women you and yours are to hold forever [as tenants] . . . to do with freely as you will.

They were to move, with all their goods, under the king's special safeguard.²¹

Simon Pérez of Foces received a section in Benejama for this colonial purpose. In 1258 he outlined a housing project for his Muslim pioneers. King James chartered it:

We assign as your own property frank and free, to you Simon Pérez of Foces and to yours forever, one open space for making and building houses in the village of Benejama in the district of Almizra; in which space and houses you are to settle men and women Saracens, however many and whomever you wish and to whomever they belong.

Here the king clearly overrode seignorial, ecclesiastical, or other jurisdictions, confirming the freedom of movement enjoyed by Muslim farmers (*exarici*) and townsmen alike. He seems to have anticipated only intra-Valencian migration here. At about the same time King James licensed Bernard of Claramunt "to populate Saracens on your estate" and took Bernard's Muslims "under our guard and protection."²²

In 1257 at the height of al-Azraq's revolt, when giving to the baron Carroz of Rebollet the castles and regions of Jalón and Vall de Laguart, the king stipulated in the settlement charter "that you can give and establish temporarily or in perpetuity houses and farms in the aforesaid castles and towns, and in their villages and districts, to Saracen men and women only." Awarding the village of Alcocer, near Cocentaina, to Martin Ximénez of Soraure in 1258, King James exempted from tax there and on Martin's Alcoy estate, "forever all Saracens who have been settled and whom from now on you will settle." A final example from this flurry of settlement projects during this disturbed period of the late 1250s was the complicated transfer of an Alcira colony. The owners had lost the area by neglecting to take up residence, but they illegally sold it to William of Llauro, who then went to great expense improving it. The king first seized it as forfeited "and gave

²⁰ King James, pregrant of Tales, Mar. 13, 1225, no. 22 in "Colección de cartas pueblas," *Boletín de la sociedad castellonense de cultura*, 11 (1930): 88-89; carta puebla de Tales, May 27, 1260, no. 84 in *ibid.*, 28 (1952): 437-38.

²¹ King James I to Bernard of Juneda, May 31, 1258, Arch. Crown, James I, Reg. Canc. 10, fol. 79.

²² King James I to Simon Pérez of Foces, June 28, 1258, *ibid.*, fol. 182; King James I to Bernard of Claramunt, Sept. 29, 1259, *ibid.*, Reg. Canc. 11, fol. 151v.

it to Saracens" but then in consideration of William's expenditures turned it back to him as landlord.²³

A strange mandate went from the Crown in 1268 to two tax farmers, the knight Roderick Martin of Azagra and his partner the Crown agent Peter Diego. They received all the revenues of the Cocentina Moors for five years, in return for a total payment of 10,000 solidi to the king in biannual installments, but on condition "that you so improve the said Moorish section that at the end of the aforesaid five years you restore to us or ours the said improved Moorish section with double the number of Saracens that reside there today." Failure to double the population would incur a penalty of 3,000 solidi. Probably most Muslim settlement was on the plan of expanding already existing *aljamas* or of bringing in as many individuals as feasible. Alfonso of Murcia may fit this pattern. Around 1264 he secured a permit for his modest settlement of Enoa; King James allowed him "full permission . . . that you may have in your township [*alcheria*] . . . twelve Saracens who are to cultivate your township; and we take the aforesaid Saracens under our protection as of now."²⁴

In 1268 King James chartered William of Rocafull, a sometime Crown lieutenant at Montpellier, to colonize his Fortaleny estates "with twenty households of Saracens." The decree given in 1274 to Raymond of Balbs from Ripoll, castellan at Calpe, had a more general effect: "While you hold the said castle for us you may . . . establish both Christians and Saracens on your estates." A directive of 1279 instructed the Jewish functionary Samuel to "establish Christians and Saracens, or Christians alone, or Saracens alone, as may seem best to you and for our advantage" at Alcudia, Favara, and Benibochir.²⁵

Some of the instruments issued by the Crown related more indirectly to the immigration movement. A whole genre concerned itself with inviting rebels and fugitives to return after civil disturbances.²⁶ Property grants to

²³ King James I to Carroz of Rebollet, Sept. 19, 1257, *ibid.*, Reg. Canc. 9, fol. 39v; King James I to Martin Ximénez of Soraure, July 1, 1258, *ibid.*, Reg. Canc. 10, fols. 83-83v; King James I to William of Llauro, Oct. 4, 1257, *ibid.*, Reg. Canc. 9, fol. 42v. In 1258 James granted to John Mur [de Muro] fourteen houses with farms "in alqueria que vocatur Nahuges que est de termino de Sexona," anticipating growth of the resident Muslim population by incoming Moors "qui . . . de cetero fuerint." King James I to John Mur, Feb. 13, 1258, *ibid.*, fol. 55v.

²⁴ King James I to Roderick Martin of Azagra and Peter Diego, Feb. 10, 1268, *ibid.*, Reg. Canc. 15, fol. 136; King James I to Alfonso of Murcia, July 22, 1264, *ibid.*, Reg. Canc. 13, fol. 203.

²⁵ King James I to William of Rocafull, Aug. 27, 1268, *ibid.*, Reg. Canc. 15, fol. 115v: "viginti casatas"; King James I to Raymond of Balbs from Ripoll, Jan. 4, 1274, *ibid.*, Reg. Canc. 20, fol. 204v; King Peter III to Samuel, Dec. 4, 1279, *ibid.*, Peter III, Reg. Canc. 42, fol. 186v. See also King James I to the bailiff of Denia, June 6, 1273, *ibid.*, James I, Reg. Canc. 21, fol. 141v: "christianis iudeis et sarracenis ad utilitatem domos et operatoria et plateas et alias terras que et quas in Denia et in Calpi et in terminis suis inveneritis ad donandum"; and King James I to Teresa Giles of Vidaure and colleagues, Mar. 27, 1261, *ibid.*, Reg. Canc. 11, fols. 197v-98: "eas donetis christianis vel sarracenis ad laborandum, prout melius vobis videbitur expedire, qui ibidem residentiam faciant et vicinaticum, et eas laborent."

²⁶ See, for example, revenue accounts by Ade of Paterna to King James I, Apr. 27, 1266, *ibid.*, Reg. Canc. 17, fol. 67: "redditus de Pego, vi millia solidorum, de quibus dimisit ut redirent sarraceni, ccc solidos." See also Prince Peter (later Peter III) to the Muslims of Pego Valley, Dec. 5, 1272, *ibid.*, Reg. Canc. 37, fol. 56v: "universis sarracenis vallis nostre de Pego et successoribus vestris illas donationes seu concessionones quas Iacobus de Linariis baiulus noster . . . fecit usque in presentem diem"; King Peter III to Muslims of Seta, July 20, 1279, *ibid.*, Peter III,

single Mudejars or to groups, either in postcrusade distribution or by special favor, frequently fall under the rubric of colonial settlement.²⁷ Moreover resettlement assumed a variety of guises. In 1274 King James threw open lands along the Alcira irrigation system to "all and single Saracen" immigrants. In 1259 he licensed the *qā'id* of Montesa to "populate with Saracens" a Játiva village.²⁸ *Aljamas* expanded, as did affluent Mudejar landholders, by substantial purchases.²⁹ King Peter continued his father's work, calling in Muslims at, for example, Beniopa, Bocairente, a dozen towns in the Denia region, and Valencia city; he even ejected Christian squatters from lands "awarded to Saracens settling."³⁰ Like his father, too, he often licensed settlement for mixed "Christians and Saracens" indifferently.

Many a settlement effort is revealed by a single document, but for Villarreal a small cartulary of six letters can be assembled. Here we watch the Crown check and clear previous title, offer the lure of tax relief, and even construct for each immigrant "one house roofed over and one corral enclosed by mud walls."³¹ A run of documents for the underpopulated

Reg. Canc. 44, fol. 149: "qui nunc ibi sunt et ex nunc ad populandum ibi possint adorare et legere in meçquitis eorum," at Seta, with general amnesty; King Peter III to Muslim settlers at Rafelbuñol, July 15, 1280, *ibid.*, fol. 185: "et universis sarracenis habitantibus et de cetero habitantes in Raffalbuynol . . . salvi, quieti, et securi." See also note 33 below.

²⁷ See, for example, King James I, directive of June 30, 1269, Arch. Cat. Val., parchment 5.997, a severe order "quod alamini sarracenorum villarum et locorum regni Valencie" title on granted or purchased property, "alias quod distringantur" by the general bailiffs of Valencia city and Játiva; and King James I to his crossbowman Sa'd, June 25, 1272, Arch. Crown, James I, Reg. Canc. 37, fol. 48: "confirmamus vobis Çaat Abeniali ballistario sarraceno nostro de Pego domos et hereditatem quem tu [*sic*] tenes in valle nostra de Pego . . . ; tu vero teneas ibi facere residenciam personalem." See also doc. of June 30, 1272, in Roque Chabás, ed., "Sección de documentos," *El archivo*, 4 (1890): doc. 37: "in quam videlicet locavimus iam sarracenis" a stretch of vineyards. The Repartimiento de Valencia records any number of such gifts. Prospero de Bofarull *et al.*, eds. *Colección de documentos inéditos del archivo general de la corona de Aragón* (Barcelona, 1847-1910), 11: 177, 184, 195, 225, 248, 271-72, 285, 299, 343, 377, 483-84, 497, 508, 568, 626, 632.

²⁸ King James I, tax exemption to Muslim settlers, Apr. 16, 1274, Arch. Crown, James I, Reg. Canc. 19, fol. 124: "sarracenos qui populabunt in populacione quam facimus subtus cequiam Algezire"; King James I to *qā'id* of Montesa, Sept. 29, 1259, *ibid.*, Reg. Canc. 11, fol. 152: "populetis et detis ad populandum sarracenis."

²⁹ Prince Peter (later Peter III) to the Muslims of Pego Valley, Dec. 22, 1272, *ibid.*, Reg. Canc. 37, fol. 57v: "toti universitati sarracenorum vallis de Pego hereditatum quam vos emisisti a Petro Mir cive Valencie in predicta valle."

³⁰ King Peter III to Raymond of Sant Leir, Apr. 21, 1279, *ibid.*, Peter III, Reg. Canc. 46, fol. 6v: "sarraceni loci eiusdem et qui ibi venerint ad populandum"; King Peter III to Peter of Bolcha, June 11, 1281, *ibid.*, Reg. Canc. 49, fol. 103: "universis sarracenis volentibus venire ad populandum apud locum nostrum de Bocayren." King Peter III to Muslim settlers from Ibi and elsewhere, Mar. 16, 1277, *ibid.*, Reg. Canc. 40, fol. 74: "populamus vos universos et singulos sarracenos de Ibi et quoscumque alios sarracenos qui in Deniam veniatis ad populandum ad cunam sarracenorum . . . et enfranquimus similiter omnes sarracenos qui ibidem venerint ad populandum a die qua venerint usque ad unum annum continuum subsequentem." On the Denia region see also King Peter III to Muslims from Moncanet, Nov. 14, 1279, *ibid.*, Reg. Canc. 44, fol. 160, and King Peter III to Moses of Alcira, Dec. 22, 1279, *ibid.*, Reg. Canc. 42, fol. 193: "sarracenis populantibus in dicto loco."

³¹ King Peter III to Muslims from Castalla and Biar, Sept. 12, 1279, *ibid.*, Reg. Canc. 42, fol. 137: to the Muslims "in frontaria de Castalla et de Biar." For Villarreal see the charters of King Peter III: Feb. 22, 1280, *ibid.*, fol. 222; Jan. 10, 1281, *ibid.*, Reg. Canc. 52, fol. 19v: "cuicumque sarraceno venienti ad populandum predictum locum de villa Regali faciatis in arraval eiusdem loci unam domum cohoptum et unum corrale clausum de tapiis"; and May 13, 1280, *ibid.*, Reg. Canc. 48, fol. 20v; see also May 13, 1280, *ibid.*, fol. 169v, and Mar. 31, 1277, *ibid.*, Reg. Canc. 39, fol. 177v.

quarters at Valencia and Játiva, capital cities for the north and south respectively, betrays similar zeal from the reign of King James through that of Alfonso.³²

Correlation between the chronologies of revolt and immigration yield no firm conclusion as to causal connection, since both movements characterized each decade from the 1240s through the late 1270s. Geographical correlation of both phenomena, during the period 1277 into 1281 for example, does not encourage the hypothesis of connection as the general explanation; too many settlements like Alcira and Villarreal were outside the zones of rebellion, and too many rebel centers do not figure in the surviving documents. The particular and general amnesties do less to explain Muslim immigration than to illustrate the sustained Christian desire for it. Postrebellion activity formed part of a continuing policy, which required more zealous implementation in periods of expulsion or mistrust.

A NUMBER OF DOCUMENTS specify Islamic lands as a source of immigrants. King Peter invited Muslim fugitives back "from any kingdoms or regions" and expressly from "Saracen" countries. His successor Alfonso similarly sought not only *los acoleiats* but also the "foreign Saracens."³³ Much of the documentation, on the other hand, might be understood either as truly foreign immigration or else as a shuffling of population within the realms. Which model dominated here, the intramural or the international? For a new kingdom only just conquered and comprising little more than a Christian-garrisoned land of aliens, such a distinction is more formal than real. In any case, given the scope of the movement, the diversity of Islamic Valencian locales, and the extent of the conquered area, the restrictive interpretation would still leave a remarkable migratory movement.

³² King Peter III to the bailiff of Valencia, Sept. 6, 1280, *ibid.*, Reg. Canc. 44, fol. 190v: "universos et singulos sarracenos venientes ad populandum et habitandum ad moreriam Valencie"; King Peter III, charter of appointment, Sept. 6, 1280, *ibid.*, Reg. Canc. 48, fol. 190v. See also the documents in Francisco A. Roca Traver, "Un siglo de vida mudéjar en la Valencia medieval (1238-1338)," *Estudios de edad media de la corona de Aragón*, 5 (1952): app. doc. 7, Feb. 16, 1277: "qui venerint ab habitandum ad revallum Valencie"; doc. 8, May 5, 1278: "qui sunt et venient ad populandum in moraria Valencie"; doc. 25, Sept. 9, 1290: "ut melius dicta moraria populeter." An example of the similar set of documents for Játiva is in note 33. Alfonso also settled "viginti casatos sarracenorum" in the Peñíscola district. King Alfonso III to William of Coret, Oct. 15, 1286, Arch. Crown, Alfonso III, Reg. Canc. 64, fol. 129.

³³ King Peter III to the Muslims of Quart, July 16, 1279, Arch. Crown, Peter III, Reg. Canc. 42, fol. 149v: "si aliquis sarracenus vel sarracena de terra regis affugerit ad loca sarracenorum et de ipsis partibus reverti voluerit ad populandum ad Quart"; King Alfonso III to the Muslims of Játiva, May 3, 1287, *ibid.*, Alfonso III, Reg. Canc. 75, fol. 5: "concedentes omnes tam sarraceni nostri . . . quam extranci qui venerint ad populandum"; this was to settle "ravallum ipsius loci Xative et alia loca nostra." An *exea*, usually employed for protecting foreign Muslims, guided them. See Reg. Canc. 74, fol. 26. See also King Peter III to officials throughout his realms, Jan. 1, 1277, in Ferran Soldevila, *Pere el Gran* (Barcelona, 1950-62), pt. 2, vol. 1, doc. 51: "quatenus si contingat aliquos sarracenos fugitivos venire ad loca nostra ab aliquibus regnis seu partibus, eosdem emparetis a quibuscumque teneantur, et eosdem salvetis, manuteneatis, et deffendatis . . . et permitatis ipsos esse in morariis salve et secure, sicut alios sarracenos." Though not without ambiguity, this seems to refer to Valencian fugitives, Crown and non-Crown, seeking to settle across the border in Aragon proper.

The restrictive interpretation also raises the question of where intramural populations came from: the immigrant direction was toward baronial as well as Crown lands, toward urban districts where Christians settled as well as toward the countryside. Towns were not left standing half empty as the result of the movement, nor did landowners raise protest that their own areas were being denuded by the invitations of competitors. Some documents concern local migration or expansion, but they do not seem to typify the general movement. It is significant that with the exception of these occasional special projects the invitations do not specify Mudejar origin or restrict immigration to recently conquered natives. Some migrants came from the older provinces of James's federated realms or from Castilian zones of reconquest, but not in sufficient numbers to evoke repercussions at the point of origin.³⁴

After its definitive conquest in 1266, and partly owing to the very different patterns of land distribution and exploitation by the Castilians, neighboring Murcia lost a substantial portion of its Muslim population;³⁵ Valencia may well have seemed a more inviting alternative than faraway North Africa in the eyes of the average poor farmer, especially in light of the Spanish chauvinism that distinguished these Muslims. Great numbers of Murcians went to Tunis, however, and anyway, Valencia's immigrant influx was notable as early as 1240 and highly visible in the fifties. On the other hand, these were decades of despair and turmoil in many parts of Mediterranean Islam; the last Abbasid caliph of Baghdad fell in 1258 before the irresistible Mongol hordes, while the last Almohad caliph of western Islam fled from Marrakesh in 1269 after decades of civil wars and fragmented principalities in North Africa. The highly mobile Islamic society surely sent streams of refugees in all directions.³⁶ Superficially it might seem that hardly any Muslims would make their way from North Africa up to Valencia, but in view of the extraordinary detente and then alliance between King James's realms and Hafsids Tunisia (Ifriqiya), and with the steady circular interchange of traders, diplomats, soldiers, and travelers that was making eastern

³⁴ Xavier le Cour Grandmaison found that nearly all Mudejar emigrants from the Balearics went to North Africa or Granada, but that over ten per cent each year chose Valencia, a percentage not far below that which the Granada group represented. "De Majorque au Maghrib, l'émigration des musulmans de Majorque entre 1344 y 1381" (Ph.D. dissertation, University of Algiers, 1966). See Charles Dufourcq, "Les relations de la péninsule ibérique et de l'Afrique du nord au xive siècle," *Anuario de estudios medievales*, 7 (1971): 57-58.

³⁵ Juan Torres Fontes, "Los mudéjares murcianos en el siglo xiii," *Murgetana*, 17 (1961): 16, 24, 26-27; very few Muslims were settled in those regions by the Castilian Crown (p. 20).

³⁶ "Mediterranean man in the Middle Ages was an impassioned and persevering traveler" even in normal times. For centuries the sea trip between places like Egypt and Islamic Spain had been a "humdrum" event for businessmen, and "commuting regularly" between the eastern and western ends of the Mediterranean was "nothing exceptional." Many ordinary folk emigrated in search of a livelihood, though naturalization required long residence and complications. Upheavals like the Almohad breakup "caused mass emigrations." For the Islamic and even the European scene in general, see S. D. Goitein, *A Mediterranean Society: The Jewish Communities of the Arab World As Portrayed in the Documents of the Cairo Geniza* (Berkeley, 1967-), 1: 42-70, 53, 57, 59, 273-81.

Barbary "a financial province of the Catalans' commercial empire," such a possibility must be seriously entertained.³⁷

Some may have made their way by sea, in the flux of alien merchants and tourists reflected in the earliest Valencian customs lists. Poor folk or the adventurous may well have been attracted by free land and the region's fabled charms, since the rural environment remained as yet so resolutely Islamic and the web of Tunisian-Catalan understandings and interchange reinforced the privileged Mudejar state-within-a-state. The crusaders who rented armies to North African potentates and who welcomed back all *alguebers* or fugitives from their panicky exile on those far shores would hardly have refused immigration applicants from overseas.³⁸ Pope Innocent's condemnation of Muslim settlement in Valencia, in fact, indifferently damns in the same breath barons "who settle Saracens or bring them back." Any number of personal names in Valencian crusader records indicate such alien connections, though without interpretative context. When an official bears the name of Muḥammad of Salā or Mūsā of Morocco, or when "a certain Saracen from Murcia" owns a small town, their personal histories remain obscure.³⁹

Paradoxically there is more evidence of increases in overseas immigration from the next century, when settlers were less welcome and therefore fenced by restrictions; thus, in the early fourteenth century an illegal entrant from North Africa was caught after residing peacefully for a year.⁴⁰ Postcrusade

³⁷ Charles Dufourcq explores the whole range of interchange, with the single exception of the movement under discussion, in his magisterial *L'Espagne catalane et le Maghrib aux xiii^e et xiv^e siècles, de la bataille de Las Navas de Tolosa (1212) à l'avènement du sultan mérinide Abou-l-Hasan (1331)* (Paris, 1966), especially chs. 2, 3; see for example pages 69–76; the summation on pages 128–31, with its characterization of Ifriqiya as "une province financière de l'empire commercial des Catalans"; and the assessment of cordiality versus antagonism on pages 185–89. On the missionary penetration of Islam, see also Burns, "Christian-Islamic Confrontation in the West"; and see Burns, "Renegades, Adventurers, and Sharp Businessmen: The Thirteenth-Century Spaniard in the Cause of Islam," *Catholic Historical Review*, 58 (1972): 341–66.

³⁸ Miguel Gual Camarena has gathered the early tariff lists of the Valencian kingdom in his *Vocabulario del comercio medieval, colección de aranceles aduaneros de la corona de Aragón (siglos xiii y xiv)* (Tarragona, 1968). For Muslims see doc. 3, Sept. 24, 1243, for Valencia city, no. 77: "sarracenus qui transeat per mare vel per terram," and no. 78; doc. 6, Mar. 10, 1250, for Alcira, no. 66; doc. 7, Sept. 1, 1251, for Biar, Burriana, Játiva, and Murviedro, no. 58: "sarracenus qui transeat . . . causa vendendi," no. 59: "sarracenus de redemptione," and no. 67: "sarracenus alforre [liber], qui exeat de Regno Valencie"; doc. 12, 1271, for Valencia city, nos. 86, 87, 96, 145; doc. 13, 1271, for Valencia city, nos. 86, 87, 95, 151. An important element in Valencian customs duties, of course, was the "traffic in emigration" as well as in slavery and redemption; see Dufourcq, *L'Espagne catalane et le Maghrib*, 76–82. On this traffic and on the *alguebers*, see Burns, "Expelling the Muslims from Thirteenth-Century Spain."

³⁹ King James I, tax record, Apr. 27, 1266, Arch. Crown, James I, Reg. Canc. 17, fol. 66: "de alqueria cuiusdam sarraceni de Murcia." For Muḥammad and Mūsā, see Burns, *Islam under the Crusaders*, 238, 399.

⁴⁰ Johannes Vincke, "Königtum und Sklaverei im aragonischen Staatenbund während des 14. Jahrhunderts," *Gesammelte Aufsätze zur Kulturgeschichte Spaniens*, 25 (1970): app., doc. 43. Such entrants were few in Valencia, compared to the Granadan flow, because the land approach was less guarded than the sea. Even in the sixteenth century, Muslim immigrants came from abroad, though their pseudo-Christian status made this easier. Witnesses for the inquisitorial trial of Cosme b. Āmir (Abenamir) included small-town Valencian Moriscos who had been natives of "Barbary," Cairo, Morocco, and Tlemcen. See the trial record (1567) in Pascual

Valencia, a more stable Islamic community than either Almohad Spain in its precrusade decades or much of North Africa for some time after the crusade period, must have attracted more settlers. Ironically, one clearly North African immigrant during this postcrusade period was a returning exile. A contemporary government figure describes this incident from King Peter's diplomatic-military expedition to al-Qull on the Tunisian coast. "A Saracen who had belonged to the kingdom of Valencia" and was in fact "a native of the valley of Alfandech" volunteered to spy on the Tunisians among whom he currently resided; as a reward Peter gave him twenty gold pieces plus a grant of land "in your birthplace, among your friends."⁴¹

The most obvious place of origin for outside Muslims, as well as point of transit, was Nasrid Granada. Here the frontier between Christendom and Islam lay startlingly open, despite all laws designed to control it, if one can judge from the easy flow of Muslim population in both directions during the better-documented and more rigidly controlled fourteenth and fifteenth centuries. A partial record of this later two-way movement is the body of petitions by returning Valencians, to the bailiff general, for waivers of prosecution. These routinely granted waivers protected illegal emigrants, upon their return to their native Valencian town, from troublesome local authorities. A recent student of this flux finds that "although nominally a frontier existed," in practice the interchange "canceled out" any political division. Returning migrants, as their numerous waivers spell out, belonged "to all the places of the Valencian kingdom," a geographical span that underlines the universal freedom of movement. In later centuries, as in the thirteenth, Crown and barons welcomed them back.⁴²

Muslim immigration did not survive into the next century as a common expedient, owing partly to a hardening of attitude toward the domestic alien. Muslims continued to be valuable sources of revenue, however, and their expanding settlement was not altogether discontinued. The military order of Montesa, successors in Valencia to the suppressed Templars, colonized several districts of Perpunchent with Muslims in 1316, for example, leaving the actual division of homesteads to their revenue officer (*amīn*). A notable example was Chelva in 1370, where the widow of Jérica's lord delegated forty Muslim entrepreneurs to bring in a hundred of their fellows "to populate" there. Castellón saw an accretion of Muslims, and in 1400 the city fathers took trouble to introduce more such settlement, redoubling their efforts in 1439. At Corbera the bailiff remitted taxes for five years to lure Mudejar settlement in the town quarter and in the countryside. The fourteenth century also witnessed what Miguel Gual Camarena calls a

Boronat y Barrachina, *Los moriscos españoles y su expulsión, estudio histórico-crítico* (Valencia, 1901), 1: app., doc. 19, pp. 544-45.

⁴¹ Ramón Muntaner, *Crònica*, ed. Enrique Bagué (Barcelona, 1927-52), ch. 53.

⁴² Leopoldo Piles Ros, *Estudio documental sobre el bayle general de Valencia, su autoridad y jurisdicción* (Valencia, 1970), 40-41, with illustrative documents as indicated.

"migratory flow" from seignorial lands onto the more tolerable Crown areas; this provoked protest by the lords and reflexive bettering of conditions on their estates, resulting in a contrary but smaller flow back onto baronial properties.⁴³

MUSLIM IMMIGRATION INTO VALENCIA began from the first days of James's conquest, rapidly attained proportions making protest futile, and then continued stronger than ever under Peter. Rome named as culprits the barons, the religious, and even the king; yet not a document has survived from those early settlements that so stirred bishops and popes. It is clear that the later documents which survive cannot be studied in isolation but must be viewed as fragments of a larger mosaic whose outlines alone are visible. Of itself this conclusion would leave a fascinating footnote to crusade history, a bizarre if hardly central feature of medieval Arago-Catalonia, and a useful insight into the evolution of Valencian Mudejarism.

The movement can serve, however, as something much more valuable for the historian—a key to deeper understanding of the crusader's mind as well as of the nature of the conquered kingdom's society. Valencia had not been only a territory to conquer but a complicated, enormously valuable economic prize. To draw from it ready advantage the crusaders had to cherish the native labor force, replacing and augmenting it. Town industries like paper, ceramics, dyes, and shipbuilding demanded skilled workers, but so did the agricultural countryside. Proper exploitation of intensely irrigated areas, islands in a sea of aridity, and of the viticulture prominent in land grants called for experienced hands precisely during the postconquest generation. James and Peter devoted attention to existing irrigation systems, even expanding them. Christian workers were not available in any number, either from the realms of Aragon or from Languedoc and the wider reaches of Christendom. The Crown's active recruitment of Jewish population could not begin to meet the need.⁴⁴ Muslims had to do. The zealous crusader, while

⁴³ Libro registro de poblaciones i privilegios, 1316, Archivo Histórico Nacional, Madrid, Ords. milit., Montesa, 542-C, fol. xix: "prout per alaminum nostrum sunt vobis assignate"; carta puebla de Chelva, Aug. 17, 1370, in Fernández y González, *Estado social y político de los mudejares de Castilla*, app., doc. 71; Vicente Traver Tomás, *Antigüedades de Castellón de la Plana, estudios histórico-monográficos de la villa y su vecindario riqueza y monumentos* (Castellón de la Plana, 1958), 187-88; on Corbera see Leopoldo Piles Ros, "La situación social de los moros de realengo en la Valencia del siglo xv," *Estudios de historia social de España*, 1 (1949): 235; Miguel Gual Camarena, "Los mudéjares valencianos en la época del Magnánimo," *IV Congrés d'història de la corona d'Aragó* (Palma de Mallorca, 1959-70), 1: 471; and see Gual Camarena, "Mudéjares valencianos," 173-74.

⁴⁴ King James encouraged Jewish immigration; on June 11, 1247, for example, he issued a safeguard to a large family of Jewish settlers and to all Jews traveling to colonize Majorca, Barcelona, and Valencia. Described in Miret y Sans, *Itinerari*, 188. Jews in James's realms were not merely townsmen but commonly agriculturalists at various levels, differing in this from their northern brothers. For a brief overview of King James's colonization policy with the Jews in Valencia, see Yitzhak Baer, *A History of the Jews in Christian Spain*, tr. Louis Schoffman et al. (Philadelphia, 1966), 1: 139-41.

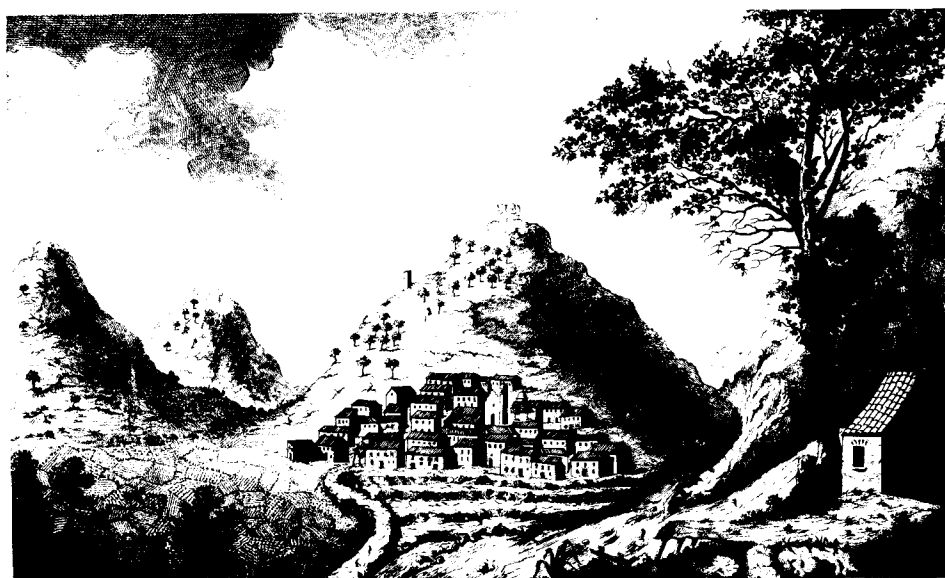


Fig. 4. A view of Eslida in the north-central portion of Valencia, inland and southwest of Castellón. This was a strong Mudejar center in the thirteenth century. Although eighteenth century, the drawing conveys the shape and nature of the Valencia farmer's landscape before radical changes altered it from any semblance of its medieval original. Note particularly the pattern of the fields. The drawing is by Antonio Josef Cavanilles (1745–1804), a noted botanist, and engraved by T. Enguidanos. From Cavanilles, *Observaciones sobre la historia natural, geografía, agricultura, población y frutos del reyno de Valencia* (Madrid, 1795–97), 2: 109.

elaborating his propaganda about expelling Mudejars and depaganizing the Islamic atmosphere, faced up to a quite different reality. Medieval men, like modern, contrived to live with contradiction.

Why did the Crown not meet the problem by increased importation of slaves? Since Christian and free Muslim farmers lived at a level of parity, a slave system had the advantage of subordinating the Moorish majority and reducing them in a way to nonpersons, thus contributing to the “Christianizing” or colonization of the realm. Joaquín Miret y Sans, a close student of slavery in the medieval realms of Aragon, has emphasized how the acquisition of considerable territory, in addition to the remarkable expansion of commerce and industry consequent upon the conquests of Majorca and Valencia, made some employment of slaves “little less than indispensable.” This was in keeping with the times; slavery, handmaid of civilization, had already established itself in the position it was to maintain down into the eighteenth century. In mid-thirteenth-century Valencia, however, the flood-gates of inexpensive slavery had just closed. Valencia continued to be an export center for the Mediterranean slave trade, and rebellions or raids occasionally provided free slaves, but the very success of the crusades by Arago-Catalonia and Castile meant an end to the stream of prisoners of war. With this main source of what Miret y Sans calls “the forced immigration of labor” shut off, peace precipitated in expansive Valencia a kind of

energy crisis. Though seignorial documents are scarce, a careful review of Templar and Hospitaller documents for the Arago-Catalan realms at this time shows a decided decline in acquisition of slaves and in cash reserves available for their purchase. The triumph of the *reconquista* spelled the landlord's misfortune.⁴⁵

The Valencian situation was not strictly a matter of economics overriding ideals. The foundations for Christian policy here were quite different, particularly the inherited Mediterranean tradition of enclave societies for conquered minorities as developed in Mudejarism. The profit motive had always buttressed this otherwise reasonable policy. In the dynamics of Valencia's peculiar maximum Mudejarism, however, the profit factor came to overshadow other elements. The gentle paradox inherent in the older expressions of Mudejar policy tended to become in this grosser form an intolerable contradiction. The crusader, no longer able to pose as a traditionalist enjoying his reasonable rewards, now seemed a greedy betrayer, perpetuating the overwhelmingly Islamic ambience for mere love of money. Pope Clement IV, in a philippic against this Valencian form of Mudejarism in 1266, realized that the strongest argument he must counter was: "on account of financial necessity." Against it Clement marshaled the good of the state, the purity of religion, and the preservation of King James's human fame.⁴⁶

James himself recognized the nature of the dilemma. When he finally determined to expel some portion of Valencia's Mudejars after the rebellion of al-Azraq, the prelates and cities yielded to persuasion, but the barons fought expulsion as economic catastrophe. James tells the story in a passage of his memoirs. Having appealed to the better nature of the barons, to their personal loyalty, and to their religious feelings, he anticipated acquiescence; so he tells us anyway, though James had an instinct for placing himself to the right of the angels and easily expressed consternation over an opponent's failings. To his dismay, "those who had Saracen vassals spoke with reluctance, for what I proposed did not seem to please them." James expressed sympathy with the barons, "because your revenues will be lessened, and not be so great by Christians as by Saracens." The barons' crisis was in fact more serious, as he well knew, with Christian replacements unavailable. Under pressure from Rome, and with his crusader image in Christendom in peril of tarnishing, King James may well have felt secret relief over the intran-

⁴⁵ Joaquín Miret y Sans, "La esclavitud en Cataluña en los últimos tiempos de la edad media," *Revue hispanique*, 41 (1917): 4; Dufourcq, *L'Espagne catalane et le Maghrib*, 71-76, 466-68. Charles Verlinden's monumental *L'esclavage dans l'Europe médiévale*, whose only volume to date covers *Péninsule ibérique, France* (Bruges, 1955), is invaluable for background but can be misleading for the general situation in Valencia. Vicenta Cortes, *La esclavitud durante el reinado de los reyes católicos (1479-1516)* (Valencia, 1964), has a bibliography for thirteenth-century antecedents; Vincke, "Königtum und Sklaverei," covers fourteenth-century Arago-Catalonia.

⁴⁶ This is not a register but the codex "Magistri Berardi [sic] de Neapoli subd. et not. ap. collectio epistolarum summorum pontificum," 1266, Arch. Vat., Reg. Vat. 29A (Clement IV, epistolae), fols. 10-11v, letter 18.

sigence of his barons. Pleading that intransigence, he saved his public image, his conscience, and his cash. Conscience revived to haunt him on his death-bed. In a codicil to his last testament King James exhorted his son and successor not to keep the Mudejar populations in Valencia, "neither ours nor theirs, for the sake of money or rent or income."⁴⁷

From the Mudejars' point of view the Christian dilemma was a boon. It opened up to many Muslims opportunities for relative affluence, conferred exemptions and privileges, extended or maintained the Islamic community, formed a bond of self-interest with the conquerors, and helped balance the trend toward second-class citizenship. These blessings grew, of course, out of the misfortune of fellow Muslims. They were predicated on a substantial drift of freer spirits into permanent exile and in part on failed rebellion or on traitorous support of the crusader cause. Though the settlement projects were in no way exploitative, they strengthened the infidel regime and tightened its inhibiting frame around the Muslim community. In cases like Biar they diluted Islamic military potential at strategic points and redistributed its Muslims to the advantage of the conquerors. They probably further distorted the balance of rural over city Moors, already inaugurated by the expulsions at Valencia and Burriana and by the tendency of Christian immigrants to cluster in the cities. They removed some Moors from heavily Islamic and therefore cohesive environments into a demographic ecology that rendered these Muslims visibly a minority.

All this can hardly have been without acculturative effect—in inducing regressive traditionalism, in bonding the Mudejars more firmly into the aliens' system, in mingling the two peoples, and in introducing a more decisive element of mobility, including upward social mobility. Since Valencian Islam had been so varied a society—comprising areas of diverse ethnic origins, differing work models, idiosyncratic local political evolutions, and opposed group psychologies stretching to antipathy—the shifting of Mudejar populations even within the kingdom's framework certainly caused repercussions in the subject people as a whole.⁴⁸ There is food here for investigation by sociologist, anthropologist, demographer, Islamologist, and historian.

⁴⁷ King James I, *Crònica (Llibre dels feyts)*, ed. J. M. de Casacuberta (Barcelona, 1926–62), chs. 366–67; King James I, will of 1276, in Charles de Tourtoulon, *Don Jaime I el Conquistador, rey de Aragón, conde de Barcelona, señor de Montpellier, según las crónicas y documentos inéditos*, rev. and tr. Teodoro Llorente y Olivares (Valencia, 1874), app., vol. 2, p. 458.

⁴⁸ On social classes and the human geography, see Burns, *Islam under the Crusaders*, chs. 4, 5. Some hint of the complex immigration antecedents of Islamic Valencia throughout its long previous history can be gained from representative short studies. See, for example, the innovative approach of Pierre Guichard, "Le peuplement de la région de Valence aux deux premiers siècles de la domination musulmane," *Mélanges de la casa de Velázquez*, 5 (1969): 103–58; on a movement of mountain people on the eve of Valencia's *reconquista*, see Julian Ribera y Tarragó, "Musulmanes de Valencia originarios de Albarracín," in his *Opúsculos dispersos* (Tetuán, 1952), 31–33; see also Jacinto Bosch Vilá, "Establecimiento de grupos humanos norteafricanos en la península ibérica, a raíz de la invasión musulmana," *Atti del I congresso internazionale di studi nord-africani* (Cagliari, 1965), 147–61; and J. T. Monroe, introd. to his translation, *The Shu'ūbiyya in al-Andalus: The Risāla of Ibn García and Five Refutations* (Berkeley, 1970), 1–21.

The topic still lies in penumbra, awaiting deeper interpretation or even moralizing, and presenting at once the light and dark sides of the destiny awaiting the free Muslim community in crusader Spain.⁴⁹

⁴⁹ It may also serve as remote background to the current problem posed by the considerable illegal immigration of Muslims into Spain. *La Vanguardia española* for June 7, 1972, under the heading "La emigración árabe a España: Un problema desatendido," reported "unos 30,000 norteafricanos" living clandestinely in the city and county of Barcelona alone. Mostly from Algeria and Morocco, they tend to enter Algeciras and Málaga, surviving by heavy construction labor and living in the poorest parts of older Barcelona.

Feminism in the French Revolution

JANE ABRAY

FRENCH FEMINISM HAS A LONG HISTORY; its roots go back far beyond the tumult of new ideas that mark the Revolution. Since the Renaissance, indeed since the Middle Ages, French women—and men—had argued for equality of legal and political rights for the sexes. Woman's education, her economic position, and her relationship to her father and husband had all been worked over time after time.¹ In the eighteenth century intellectuals carried on a desultory debate over the status of women. The discussion slowly grew more heated until, in the early years of the Revolution, a small group of bold thinkers demanded changes that, if effected, would have altered the character of French civilization far more than did the abolition of the monarchy.

Single or married, women had few rights in the law during the last decades of the *ancien régime*. Their testimony could be accepted in criminal and civil courts but not for notarized acts like wills. In some parts of France a single woman could enter into contractual relationships, but for the most part her rights—reasonably extensive as late as the thirteenth century—had atrophied.² Generally speaking a single woman remained under her father's authority until she married; marriage transferred her to her husband's rule.³ Once married she generally had no control over her person or her property.

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¹ See Léon Abensour, *Histoire générale du féminisme des origines à nos jours* (Paris, 1921); Lulu McDowell Richardson, *The Forerunners of Feminism in the French Literature of the Renaissance from Christine of Pisa to Marie de Gournay* (Baltimore, 1929); and Georges Ascoli, "Essai sur l'histoire des idées féministes en France du XVI^e siècle à la Révolution," *Revue de synthèse historique*, 13 (1906): 25-57, 161-84. The term "féminisme" itself did not come into use in French until the nineteenth century. Charles Fourier used it first in the second edition of his *Théorie des Quatre mouvements* (Paris, 1841); see Charles Turgéon, *Le féminisme français* (Paris, 1907), 1: 10.

² Marcel Garaud, *La Révolution et l'égalité civile* (Paris, 1953), 172-74. On women's political participation and legal rights in the Middle Ages, see Maurice Bardèche, *Histoire des femmes* (Paris, 1968), 2: 60-61, 72-73.

³ Philippe Sagnac, *La législation civile de la Révolution française* (Paris, 1898), 295. For an explanation of the father's power, see Jacques Godechot, *Les institutions de la France sous la Révolution et l'Empire* (Paris, 1951), 206-07.

Only the death of her husband could offer her some prospect of independence. As Robert Joseph Pothier, an eighteenth-century legal expert, explained, "Our customary law has put women into such a condition of dependence on their husbands that they can do nothing valid, nothing that the civil law will recognize, unless they have been specifically authorized by their husbands to do it."⁴ Nor was the economic position of eighteenth-century women enviable. Although their earnings were vital to the survival of lower-class families, their wages were very low. The guild offices excluded women, and even the slight modernization of industry accomplished before the Revolution tended to worsen their condition. For the most part law and custom confined women to domestic service, heavy labor, and ill-paid labor-intensive industries like the lace trade.⁵ Surprisingly enough, French women did have some political rights. The regency was open to women. According to the king's summons of the Estates-General women in religious orders and some noblewomen could send representatives to the Estates. A few women of the Third Estate, particularly widows, managed to participate in some of the primary assemblies.⁶

This was the subordinate position that the eighteenth-century intellectuals debated. The great figures of the Enlightenment—Montesquieu, Voltaire, Rousseau, Condorcet, Diderot, and other *Encyclopédistes*—contributed to the discussion but were not its main speakers.⁷ From the middle of the century on a host of now-obscure writers took up the feminist case: Abbé Joseph-Antoine Toussaint Dinouart, Philippe Florent de Puisieux, Mlle Archambault, Pierre Joseph Caffieux, Pierre Joseph Boudier de Villemert, Mme Riccobini, [Antoine-Léonard?] Thomas, and Mme de Coicy.⁸ As ad-

⁴ Robert Joseph Pothier, *Traité de la puissance du mari* (1769), in his *Oeuvres complètes* (Paris, 1821), 10: 655.

⁵ Owen Hufton, "Women in Revolution, 1789–1796," *Past and Present*, 53 (1971): 91–92; Evelyn Sullerot, *Histoire et sociologie du travail féminin* (Paris, 1968), 69–74. In the weaving trade women were explicitly refused the right to become masters because if they were allowed to improve their status "la rareté des tireuses occasionnerait une augmentation ruineuse de la main d'oeuvre." *Ibid.*, 73.

⁶ "Règlement fait par le roi pour l'exécution des lettres de convocation du 24 janvier 1789," in *Introduction historique à l'Ancien Moniteur* (Paris, 1843), 559–60. On earlier meetings of the Estates, see Louis Franck, *Essai sur la condition politique de la femme* (Paris, 1892), 304–09. On women in the Assemblées Primaires, see the remarks of Paul-Marie Duhet about Chevenanceaux, in *Les femmes et la Révolution, 1789–1794* (Paris, 1971), 25.

⁷ On the philosophe's position, see David Williams, "The Politics of Feminism in the French Enlightenment," in Peter Hughes and David Williams, eds., *The Varied Pattern: Studies in the Eighteenth Century* (Toronto, 1971), pp. 333–51; and Edwin R. Hedman, "Early French Feminism from the Eighteenth Century to 1848" (Ph.D. dissertation, New York University, 1954). Condorcet alone held strong feminist convictions. Rousseau held equally clear objections. Montesquieu and the others show some sympathy, but none of them could be called feminists.

⁸ Abbé Joseph-Antoine Toussaint Dinouart, *Le Triomphe du Sexe* (Amsterdam, 1749); Philippe Florent de Puisieux, *La femme n'est pas inférieure à l'homme* (London, 1750) and *Le Triomphe des dames* (London, 1751), both supposedly translations of English works; Mlle Archambault, *Dissertation sur la question: Lequel de l'homme ou de la femme est plus capable de constance? Ou la Cause des dames* (Paris, 1750); Pierre Joseph Caffieux, *Défense du beau sexe* (Paris, 1753); Pierre Joseph Boudier de Villemert, *L'Ami des femmes* (n.p., 1758), with at least four more editions before the Revolution; Mme Riccobini, *L'Abeille* (1765); [Antoine-Léonard?] Thomas, *Essai sur les femmes* (Paris, 1772), with three more editions, 1772–73; Mme de Coicy, *Les femmes comme il convient de les voir* (London and Paris, 1785).

vocates of social revolution this group must be accounted tame. Boudier de Villemert maintained that women ought to have "a serious daily occupation" and recommended embroidery.⁹ Potential feminists could have found sterner stuff in the *Journal des Dames*, a monthly magazine. Its editor in 1774, Mme de Montenclos, was an advocate of women's rights. She staunchly proclaimed, "I am not out to draw attention to myself, but I swear I do want to *shatter our conventions* and guarantee women the justice that men refuse to them as if on a whim."¹⁰ Many of the opponents of these ambitions lurked in the vast literature on women's education. Restif de la Bretonne, following the path of Rousseau's *Emile*, ordered that all thought of equality between the sexes be suppressed. Women should be forbidden to learn reading and writing in order to limit them to useful domestic labor.¹¹ Mme de Genlis urged that women's education be organized to prepare them "for a monotonous and dependent life."¹² While the supporters of feminism tended to exalt marriage and motherhood as a claim on society, the anti-feminists used this same "natural vocation" to prove that women should be content to stay home and to obey their husbands.

By 1789 CONVENTIONAL WISDOMS of all sorts, and even the image of the happy homemaker, had begun to quiver. For in the last years of the decade a more militant feminist theory had emerged in a spate of pamphlets. No longer content to make vague statements advocating equality, the partisans of women's emancipation got down to specific proposals about education, economics, and legal and political rights. Their brochures began to appear in 1787 and quickly multiplied. The general argument ran: human beings are naturally equal, therefore sexual discrimination is unnatural; husband and wife should be equal partners in marriage; women ought to have a better education and access to more, and higher-paid, jobs. Along with demands for marital and economic equality the new feminism laid claim to the vote.

The Marquis de Condorcet sounded the first blast of this trumpet in favor of the regiment of women. He reasoned that women, since they were not allowed to vote, were being taxed without representation and would be justified in refusing to pay their taxes. Moreover, said Condorcet, domestic authority should be shared and all positions and professions opened to both sexes. He observed that sexual inequality was a new state and not the traditional lot of women. A year later Condorcet insisted that women who met the property qualifications he proposed for the suffrage should vote. He also

⁹ Boudier de Villemert, *L'Ami des femmes*, 51-54.

¹⁰ Quoted in Evelyn Sullerot, *Histoire de la presse féminine des origines à 1848* (Paris, 1966), 23, italics in original.

¹¹ Restif de la Bretonne, *Les Gynographes* (Paris, 1777), 92, 180.

¹² Mme de Genlis, *Adèle et Théodore ou Lettres sur l'éducation* (1782), in her *Oeuvres complètes* (Maestricht, 1782), 10: 30.

predicted that his ideas would get little support from women, as they were all too enamored of Jean-Jacques Rousseau to listen to him.¹³

Most women ignored the feminists. Yet Condorcet found some allies. Two pamphlets, *Requête des femmes pour leur admission aux Etats-Generaux* and *Remontrances, plaintes et doléances des Dames Françaises*, called for political rights; the latter also criticized men for stultifying women's minds through a too-narrow education.¹⁴ Not all of these pamphlets were concerned primarily with political rights. "We ask for enlightenment and jobs," said the women of the Third Estate to the king, "not to usurp men's authority, but to rise in their esteem and to have the means of living safe from misfortune."¹⁵

One of the most important of these early pamphlets was *Cahier des doléances et réclamations des femmes, par Mme B... B...*. The anonymous author began by revealing her astonishment that women were not seizing the opportunity to make themselves heard. She described her own conversion to feminism—she had thought women weak and incompetent but now knew better—and asked whether men could continue to make women the victims of their pride and injustice at a time when the common people were entering into their political rights and when even the blacks were to be free. She insisted that just as a noble could not represent a *roturier* in the Assembly, so a man could not represent a woman. Mme B... B... then lashed out at the double standard of sexual morality, at the *droits d'ainesse* and at those of *masculinité*.¹⁶ This pamphlet reappeared word for word as *Cahier des doléances et réclamations des Femmes du département de la Charente*.¹⁷

Other pamphlets appeared along with a flurry of satires mocking the feminists' pretensions.¹⁸ Condorcet contributed another major statement in which he repeated his earlier arguments on behalf of women's suffrage and

¹³ Marquis de Condorcet, "Lettres d'un bourgeois de New Haven à un citoyen de Virginie" (1787), in his *Oeuvres*, ed. Frank O'Connor and M. F. Arago (Paris, 1847), 9: 15–19; Condorcet, "Essai sur la constitution et les fonctions des assemblées provinciales" (1788), in *ibid.*, 8: 141–42. Rousseau's popularity with women is an indication of how few of them held any feminist convictions.

¹⁴ See Charles-Louis Chassin, *Le Génie de la Révolution* (Paris, 1863), 1: 477.

¹⁵ *Pétition des femmes du Tiers au roi, 1er janvier 1789*, extracts in Jeanne Bouvier, *Les femmes pendant la Révolution* (Paris, 1931), 249–50. Duhet compares this petition to *Cahier des doléances et réclamations des femmes, par Mme B... B...* "Les femmes et la Révolution", 32–39.

¹⁶ Bouvier, *Les femmes pendant la Révolution*, 266–74.

¹⁷ Duhet, *Les femmes et la Révolution*, 41. The pamphlet was reprinted under the second title in the *Etrennes nationales des Dames*. See Sullerot, *Histoire de la presse féminine*, 47–50.

¹⁸ *Les très humbles remontrances des femmes françaises* (1788); *De l'influence des femmes dans l'ordre civil et politique* (1789); Sophie Rémi Courtenai de la Fosse Ronde, *Argument des femmes aux Etats-Generaux* (1789); *Réponse des Femmes de Paris à l'Ordre le plus nombreux de France* (1789); *Discours préliminaire de la Pauvre Javotte* (1790). I have found no trace of the contents of the first four. The *Discours préliminaire* calls for the abolition of the clergy and the nobility as orders and the constitution of women as the new second order. It castigates the Revolution for neglecting poor women to concentrate on the "aristocratie masculine." Bouvier, *Les femmes pendant la Révolution*, 297–303. The identification of masculinity with aristocracy was a favorite device since it permitted feminists to co-opt all the ideas and stock phrases of the Third Estate's campaign. Duhet discusses some of the satires. *Les femmes et la Révolution*, 30–32.

ridiculed one of the opposition's favorite arguments. "Why should people prone to pregnancy and passing indispositions be barred from the exercise of rights no one would dream of denying those who have gout or catch cold easily?"¹⁹

A very few feminist proposals appeared in the *cahiers*. The most common of these was the appeal for improvements in the education of women.²⁰ The Third Estate of Châtellerault (Poitou) made a unique suggestion. "Let the assemblies be constituted according to an equitable procedure; accordingly let citizens of both sexes and of all ages have equal rights in participating in the debates of the assemblies and in the appointing of deputies." Far more ordinary was their suggestion that paid midwives be provided for the countryside.²¹ The drawing up of the *cahiers* also prompted comments from interested observers. One set of anonymous *Observations sur la rédaction des Cahiers de Paris* urged that men be forbidden to exercise "women's professions," thus assuring women the means of making their living and consequently keeping them from turning to prostitution. Henri Jabineau, a lawyer and abbé, sent thirty-two articles echoing these themes first to a Parisian electoral assembly and then to the Estates-General.²²

Once the Estates had met and representative government had begun, the feminists changed their tactics. No longer did they rely on pamphlets and letters to the editor. Instead they took to sending delegations to the government and to using the political clubs as platforms. Representations to the National Assembly began very early. In November 1789 the Assembly received a series of "Motions en faveur du sexe" that attacked the economic subordination of women and the evils of convent life.²³ This habit of addressing proposals directly to the government persisted at least until 1793. Mme Mouret went to the Assembly in 1790 to present a speech on the need for women's education.²⁴ Early in 1792 several Parisians of both sexes

¹⁹ Condorcet, "Sur l'admission des femmes au droit de la cité," published in the Cercle Social's *Journal de la société*, 1790, in Condorcet, *Oeuvres*, 10: 119-30.

²⁰ Elizabeth Racz says that thirty-three of the *cahiers* recommended educational opportunities for women. "The Women's Rights Movement in the French Revolution," *Science and Society*, 16 (1951-52): 153.

²¹ *Archives Parlementaires* (hereafter *AP*) (Paris, 1867-1972), 1789, vol. 2, pp. 691, 696. The government considered the need for midwives. See the meetings of July 31, Aug. 6, and Sept. 4, 1790, in *Procès-verbaux et rapports du comité de mendicité de la Constituante, 1790-91*, ed. Camille Bloch and Alexandre Tuetey (Paris, 1911), 104, 108, 168 n.1.

²² Charles-Louis Chassin, *Les élections et les cahiers de Paris en 1789* (Paris, 1888), 3: 168, 384-86. The interconnection of prostitution and women's economic condition is acknowledged in some of the *cahiers*. Racz, "Women's Rights Movement," 152. See also Richard Cobb, *The Police and the People* (London, 1970), 234-39, and his *Reactions to the French Revolution* (London, 1972), 134. I have not found much evidence of a distinctively feminist attitude to prostitution. Attacks on its evils were common throughout the Revolution; almost no one who complained about it attached any responsibility to the customer.

²³ "Motions adressées à l'Assemblée nationale en faveur du sexe," *Moniteur*, Nov. 29, 1789, reprinted in *Réimpression de l'Ancien Moniteur* (Paris, 1843-63), 2: 262-63 (hereafter cited simply as *Moniteur*, with volume and page numbers of the reprint). See also Duhet, *Les femmes et la Révolution*, 54-56.

²⁴ *Moniteur*, Mar. 26, 1790, vol. 3, p. 703. Mme Mouret's speech, published as *Annales de l'éducation du sexe ou Journal des demoiselles* (1790), is excerpted in Sullerot, *Histoire de la presse féminine*, 52-54.

requested the Assembly to pass a law against despotic paternal and marital power.²⁵ In April of that year Etta Palm van Aelders, a Dutch feminist, petitioned the Assembly to provide education for girls, to guarantee women's legal majority at twenty-one, to give both sexes political freedom and equal rights, and to present divorce legislation.²⁶ The following summer a woman from the Beaurepaire section addressed the Convention.

Citizen legislators, you have given men a Constitution; now they enjoy all the rights of free beings, but women are very far from sharing these glories. Women count for nothing in the political system. We ask for primary assemblies and, as the Constitution is based on the Rights of Man, we now demand the full exercise of these rights for ourselves.

The president congratulated her deputation for its zeal—and postponed discussion.²⁷

Such a discussion might have proved to be quite excited. The assemblies had their full complement of antifeminists, but they also contained a few advocates of women's emancipation. In 1792 Aubert-Dubayet of Isère spoke on the recording of vital statistics; he called women "the victims of their fathers' despotism and of their husbands' perfidy" and warned that French law must not maintain women in a state of slavery.²⁸ In the spring of 1793 Pierre Guyomar, from the Côtes-du-Nord, presented the Convention with his reflections on political equality. To him the only differences between men and women lay in their reproductive systems, and he could not understand why such physical differences should lead to differences before the law. Like many other contemporary feminists, Guyomar compared sexual to racial discrimination. He spoke, too, of "une aristocratie formelle des hommes."²⁹

Supporters of women's rights did not completely abandon their old platforms. Letters to newspapers continued to appear.³⁰ The founder of the *Journal des Droits de l'Homme*, a Cordelier named Labenette, defended the rights of women.³¹ A major feminist declaration arrived on the streets of Paris in 1791. Olympe de Gouges, having had enough of the "rights of man," announced the rights of women. Her text followed closely that of the declaration of August 1789.

All women are born free and remain equal to men in rights. . . . The aim of all political associations is the preservation of the natural and inalienable rights of women and men. . . . The nation is the union of women and men. . . . Law is

²⁵ *AP*, Feb. 13, 1792, vol. 38, p. 466. Nothing came of the request at this time.

²⁶ *Ibid.*, Apr. 1, 1792, vol. 41, pp. 63–64. Louis Prudhomme ridiculed the petition in "Encore une pétition de femmes," in his *Révolutions de Paris*, Mar. 31–Apr. 7, 1792, no. 143, pp. 20–24.

²⁷ *AP*, July 4, 1793, vol. 68, p. 254.

²⁸ *Ibid.*, Aug. 30, 1792, vol. 49, p. 117.

²⁹ *Ibid.*, Apr. 29, 1793, vol. 63, pp. 591–99.

³⁰ Note the correspondence in the *Courrier de l'hymen*, Feb. 1791, given in Bouvier, *Les femmes pendant la Révolution*, 110.

³¹ Sullerot, *Histoire de la presse féminine*, 61. Duhet adds that, pressured by other men, he ceased his propaganda. *Les femmes et la Révolution*, 213–15.

the expression of the general will: all female and male citizens have the right to participate personally, or through their representatives, in its formation.

De Gouges also demanded equality of opportunity in public employment, the right to paternity suits, and an end to male tyranny generally.³² The following year Mary Wollstonecraft's *Vindication of the Rights of Women*, inspired in part by the Revolution, appeared in a French translation and created some stir.³³

Women also made their presence felt in the great revolutionary *journées* and in the army.³⁴ While this activity was not, strictly speaking, feminist, any activity by women in a society that places a premium on female passivity has some feminist overtones. Nor were the implications of their actions lost on the women themselves. In 1789 the women of the Halles were singing:

A Versail' comme des fanfarons,
J'avions amené nos canons: [*bis*]
Falloit voir, quoi qu' j'étions qu'des femmes
Un courage qui n'faut pas qu'on blâme.

Nous faisons voir aux homm' de coeur
Que tout comme eux j'n'avions pas peur: [*bis*]
Fusil, musquetons sur l'épaule,
J'allions comme Amadis de Gaule.³⁵

The *Etrennes nationales des Dames*, a feminist newspaper begun in November 1789, used the same episode to threaten "aristocratic husbands" that women could just as easily take up arms against them if they persisted in their pretensions.³⁶ In January 1794, when the back of feminism had broken under the weight of public and governmental hostility, some women still remembered their old enthusiasms. A police spy reported on groups of women eager to see Reine Chapuy, a female cavalry soldier. The idea of her daring aroused several of these women to attack male cowardice and to exalt female courage.³⁷

Most of the people behind this agitation have left little trace. Some of their clubs can be pinned down; a few of the most flamboyant leaders survive as individuals. Of the Paris political clubs the Cercle Social was the first to advocate feminism. Its members began to hear radical ideas about women's place in society in October 1790; both Condorcet and Etta Palm

³² Olympe de Gouges, *Droits de la femme*, Sept. 1790, complete text in Bouvier, *Les femmes pendant la Révolution*, 283–89.

³³ Sullerot, *Histoire de la presse féminine*, 45.

³⁴ On women in the *journées*, see George Rudé, *The Crowd in the French Revolution* (Oxford, 1967). For women soldiers, see Raoul Brice, *La femme et les armées de la Révolution et de l'Empire (1792–1815)* (Paris, n.d.). Léon Schwab has reprinted a satire against the women soldiers in "Les femmes aux Armées," *La Révolution dans les Vosges*, 6 (1912–13): 109–15. The Convention tried to remove the women soldiers, but some of them managed to evade its decree. *AP*, Apr. 30, 1793, vol. 63, pp. 628–29.

³⁵ Quoted in Cornwell B. Rogers, *The Spirit of Revolution in 1789* (Princeton, 1949), 182.

³⁶ Sullerot, *Histoire de la presse féminine*, 48–49.

³⁷ Report of Charmont, Jan. 20, 1794, in Pierre Caron, ed., *Paris Pendant la Terreur: Rapports des agents secrets du ministre de l'intérieur* (Paris, 1910–64), 3: 56.

used it as a forum.³⁸ Several of the Parisian *sociétés populaires* accepted women: the Club des Indigents, Club des Halles, Club des Nomophiles, Club des Minimes, the Société Fraternelle des Jacobins, and that of the Carmes.³⁹ Although the Société Fraternelle des Jacobins, for one, had female officers, there is no evidence that any of these clubs were directly involved in feminist activities. The same holds true of the provincial women's clubs in Besançon, Bordeaux, Dijon, Orléans, Strasbourg, and elsewhere.⁴⁰ The provincial women's clubs attracted middle-class women, but in Paris the rank and file were usually from the lower classes. On the other hand the male feminists of whom we have record were generally fairly substantial citizens.

The feminist leaders about whom enough is known to permit biographical sketches were rather a curious crew.⁴¹ Olympe de Gouges—born Marie Gouze in 1748—was a failed playwright whose royalism and opposition to Robespierre combined to bring her to the guillotine. She relied on brochures, posters, long letters to newspapers, and very unpopular plays to spread her message. She had little enough influence. As a police spy described the reaction to one of her placards, "People stop a minute, then walk off saying, 'Oh, it's just Olympe de Gouges!'"⁴² There can be no doubt about the ardent feminism of the author of the *Droits de la Femme*. Although her execution in 1793 had obvious political causes, it was not without its significance as a gesture of repression toward the feminists. The semiofficial *Feuille du salut public* gloated, "It seems the law has punished this conspirator for having forgotten the virtues that suit her sex."⁴³

Etta Palm, who like de Gouges glamorized her name by declaring herself the "Baronne" d'Aelders, came to Paris from Holland in 1774. Palm urged the Constituent Assembly to form a company of amazons as "a first blow to the prejudices that have been wrapped around our lives," and she advocated "a second revolution in our customs" to overthrow sexual

³⁸ Alphonse Aulard, "Le féminisme pendant la Révolution française," *Revue bleue*, 4th ser., 9 (1898): 362-66.

³⁹ Isabelle Bourdin, *Les Sociétés populaires à Paris pendant la Révolution* (Paris, 1937), 139.

⁴⁰ There were many more women's clubs, some of which have been described in Marc de Villiers, *Histoire des clubs de femmes et des légions d'amazones* (Paris, 1910). On the groups mentioned here, see Claude Brelot, "Besançon révolutionnaire" in *Cahiers de l'Association interuniversitaire de l'Est*, no. 10, *La Révolution à Besançon et dans quelques villes de l'Est de France* (Strasbourg, 1966); Henriette Perrin, "Les clubs de femmes de Besançon," *Annales révolutionnaires*, 9 (1917): 629-53; 10 (1918): 27-63, 505-32, 645-72; Aurelien Vivie, *Histoire de la Terreur à Bordeaux* (Bordeaux, 1877); Geneviève Langeron, "Le club des femmes de Dijon pendant la Révolution," *La Révolution en Côte d'Or*, n.s. 5 (1929): 5-71; Camille Bloch, "Les femmes d'Orléans pendant la Révolution," *Révolution française*, 43 (1902): 49-62; and *Les Sociétés politiques de Strasbourg pendant les années 1790 à 1795: Extraits de leur procès-verbaux* (Strasbourg, 1865).

⁴¹ Condorcet, whose life is well known, will not be discussed here.

⁴² Report by Latour-Lamontagne, Sept. 21, 1793, in Caron, *Paris pendant la Terreur*, 1: 155.

⁴³ Quoted in Léopold Lacour, *Trois femmes de la Révolution* (Paris, 1900), 2. See also Charles Monselet, "Olympe de Gouges," in his *Les oubliés et les dédaignés* (Paris, 1859), 139-76; and the account of de Gouges's trial in Émile Campardon, *Le Tribunal révolutionnaire de Paris* (Paris, 1866), 1: 164-65.

tyranny.⁴⁴ She addressed to the general populace an *Appel aux Françaises sur la régénération des mœurs et nécessité de l'influence des femmes dans un gouvernement libre*. In 1791 she tried to organize a national federation of women's groups. Failure here did not stop her. She went on to address the Assembly, demanding equal employment and education, as well as political and legal equality. Its president replied ambiguously; the legislature would avoid taking any actions that might bring the citizens to regret and tears. Like de Gouges, Palm practiced the wrong politics—she had, for example, invited the Princesse de Bourbon to be a patron to one of her charitable organizations. Unlike de Gouges she had the good sense to leave France before the government could arrest her.⁴⁵

Théroigne de Méricourt, whose real name seems to have been Anne Terwagne, is perhaps the best known of these three, largely because of the attacks her contemporaries made on her. She created a sensation in the early years of the Revolution, holding a salon, trying to form a women's club, participating in the attacks on the Tuilleries, and striding about in riding clothes. Her feminism was something of a sideline, albeit sincere. In an autobiographical account she declared herself to be "humiliated by the servitude and the prejudices in which male vanity keeps our oppressed sex." She encouraged women to form a militia company because, she said, "it is time for women to break out of the shameful incompetence in which men's ignorance, pride, and injustice have so long held us captive." Her attempts to found a women's club provoked Antoine-Joseph Santerre, the commander of the National Guard, to observe that the men of his section would rather find their homes in order when they came back after a hard day's work than be greeted by wives fresh from meetings where they did not always gain in sweetness. Like de Gouges and Palm, Théroigne was politically moderate, a friend to the Girondin deputies. The shock from a beating she received from a group of Jacobin women in the spring of 1793 seems to have turned her mind. After spending some time in an asylum she was released, only to be permanently recommitted in 1797.⁴⁶

Two other women deserve mention, the chocolate maker Pauline Léon and the actress Claire Lacombe, founders and presidents of the most famous of the women's clubs, the Citoyennes Républicaines Révolutionnaires.

⁴⁴ Quoted in *Bouche de fer*, Jan. 3, 1791, quoted in P. J. B. Buchez and P. C. Roux, *Histoire parlementaire de la Révolution française* (Paris, 1834-38), 8: 424-27.

⁴⁵ There is no biographical study of Etta Palm. Episodes in her life are described in Aulard, "Le féminisme pendant la Révolution française," 364-65; in Villiers, *Histoire des clubs de femmes*; and in Bourdin, *Les Sociétés populaires*, 144-48, 151, 160, 289. See also Marie Cerati, *Le club des citoyennes républicaines révolutionnaires* (Paris, 1966), 19-21.

⁴⁶ Her life is given in Lacour, *Trois femmes*. Her own account of it is appended to Emma Adler, *Die Berühmten Frauen der französischen Revolution* (Vienna, 1906), 244-78. Her speech on the women's militia is quoted in Cerati, *Le club des citoyennes*, 18; that of Santerre is in Bourdin, *Les Sociétés populaires*, 153. One of Théroigne's doctors, Jean Esquirol, described her last years in *Les maladies mentales* (Brussels, 1838). The *Actes des Apôtres* (1789-91) is a fertile source of attacks on her; see in particular version 2, no. 38; version 4, no. 110; and version 6, no. 169. See also Marcellin Perlet, *Les Actes des Apôtres, 1789-91* (Paris, 1873), 145-56.

Founded in the spring of 1793, the club contributed to the fall of the Girondins, then drifted away from the Jacobins toward the *enragés*, a move that had much to do with its eventual suppression. The Républicaines were sans-culottes women, and their program emphasized economic claims, notably cheap food, rather than strictly feminist demands. Nevertheless the Républicaines showed some sympathy for women's emancipation. Only two accounts of their meetings survive, and one shows the Républicaines discussing women's capacity to govern. At the first of these the *citoyenne* Monic concluded that women were certainly worthy to rule nations, perhaps even more so than were men.⁴⁷ In June 1793 the Républicaines tried to put their ideas into practice by attempting to gain entry to the Conseil Général Révolutionnaire, newly set up in Paris.⁴⁸ The women of the Droits de l'Homme section had high praises for their activities.

You have broken one of the links in the chain of prejudice: that one, which confined women to the narrow sphere of their households, making one half of the people into passive and isolated beings, no longer exists for you. You want to take your place in the social order; apathy offends and humiliates you.⁴⁹

The feminist program for educational, economic, political, and legal change developed piecemeal. To justify their goals the feminists used three major arguments. First, women were human beings who therefore shared in the natural rights of man, a conviction often explicitly expressed but also implicit in the borrowing of political terms like "aristocracy" and "despotism" to describe the old system. Feminists saw the women's struggle as parallel to and a continuation of the war of the Third Estate against the upper classes. Second, the feminists made use of women's biological role. As the mothers of all citizens women had a special claim on the state, for they guaranteed its survival. Unlike modern feminists, they made no attempt to define women as other than mothers and potential mothers. Third, once the Revolution was under way, feminists cited women's political contributions to the struggle for liberty and pointed to their continuing patriotism. Since they were fulfilling the duties of citizens women could not logically be denied the rights of citizens. The feminists felt they had solid grounds for their proposals, but one by one the revolutionary governments rejected them.

Education was the most important feminist rallying point. It was also the subject on which feminists and their opponents had managed some agreement before the Revolution. The conviction that women's education needed improvement had been fairly general before 1789. The revolu-

⁴⁷ The account of the first meeting is in P.-J. Proussinale [Pierre-Joseph Alexis Roussel], *Le château des Tuilleries* (Paris, 1802), quoted in Cerati, *Le club des citoyennes*, 49-51. For the other account see note 98 below.

⁴⁸ *Moniteur*, May 31, 1793, vol. 16, p. 527.

⁴⁹ Quoted in Bouvier, *Les femmes pendant la Révolution*, 329-31. Scott Lytle, "The Second Sex (1793)," *Journal of Modern History*, 27 (1955): 14-26, is clearer on the Républicaines' end than is Cerati, *Le club des citoyennes*, 110-78.

tionary governments considered a multitude of educational projects from which some common principles can be extracted. Most projects followed Talleyrand's lead in declaring that both sexes must be educated and then sharply distinguishing between the kinds of education suitable to each. His "Projet de décret" read in September 1791 sounded a note that would recur again and again. "All the lessons taught in the public schools will aim particularly to train girls for the virtues of domestic life and to teach them the skills useful in raising a family."⁵⁰ The *conventionnel* Alexandre Deleyre dismissed secondary education for women as unnecessary.⁵¹ Some of the less progressive legislators would have denied even primary education to girls, preferring to see them taught housekeeping at home.⁵² The Convention's Committee on Public Education did in fact vote to suppress girls' schools in the summer of 1795 but then changed its mind the following year.⁵³ Since the revolutionary governments never succeeded in running a national educational system, it is difficult to evaluate their work and dangerous simply to assume that they neglected women. Yet it is clear that the government intended to reinforce and to perpetuate sexual differences through public education.⁵⁴ It is also clear that the Revolution was unable to improve or even to expand women's education. On the other hand the secularization of education and the promise of an expanded primary school system held out some hope of employment to literate single women.⁵⁵

This was one of the few hopes the Revolution offered to women who had to earn their own living. The Committee of Public Safety and the Con-

⁵⁰ *AP*, Sept. 10, 1791, vol. 30, pp. 449, 478-79, 499.

⁵¹ *Ibid.*, July 3, 1793, vol. 68, p. 193. See also the opinion of Charles Gilbert Romme in the Legislative Assembly, *ibid.*, Dec. 20, 1792, vol. 55, p. 191.

⁵² *Ibid.*, July 29, 1793, vol. 69, pp. 670-74.

⁵³ See the meetings of Aug. 15, 24, and Oct. 25, 1795, in *Procès-verbaux du Comité d'instruction publique de la Convention nationale*, ed. M. J. Guillaume (Paris, 1891-1907), 6: 546, 580, 873.

⁵⁴ For further evidence of this, see the following proposals in *AP*: Dec. 12, 1792, vol. 55, pp. 25-27; July 2, 1793, vol. 68, pp. 113-17; July 3, 1793, vol. 68, pp. 150-52, 179-94, 194-96; July 13, 1793, vol. 68, pp. 661-75; July 29, 1793, vol. 69, pp. 670-74, 674-79. The records of the Assembly's and Convention's education committees are less rewarding. The former promised to produce a decree on women's education, but apparently never got around to it. Meeting of Apr. 21, 1792, in *Procès-verbaux du Comité de l'instruction publique de l'Assemblée législative*, ed. M. J. Guillaume (Paris, 1889), 250. The latter demonstrates that women continued to be interested in education and to advise the government on how best to organize it. See the meetings of Oct. 12, Nov. 5, 1793, and June 19, 1794, in *Procès-verbaux du Comité d'instruction publique de la Convention nationale*, 2: 608, 746; 3: 613. One would expect Condorcet's report on education to favor feminist aspirations. After promising equal primary education, Condorcet dropped the subject, telling the Legislative Assembly that a separate report on this important subject would be presented later. *AP*, Apr. 21, 1792, vol. 42, p. 236. In the version given in his *Oeuvres* (7: 215-26) he devotes more attention to it. Here he says that women can only be taught the same truths as men and urges the opening of the sciences to them.

⁵⁵ At one point the government even offered equal pay for equal work, but this idea eventually disappeared. Convention decree, *Moniteur*, June 26, 1793, vol. 17, p. 41. Governmental authorities made pious statements on the equal importance of the work of "instituteur" and "institutrice," but there is evidence that ordinary people saw a large difference. See, for example, the case of Girard Viry, who, to his horror, was hired as an *institutrice*. Charles Henry Chevalier, "Le citoyen Girard Viry, 'Institutrice,'" *La Révolution dans les Vosges*, 18 (1930): 19-24. When it cared to do so the government could distinguish men from women. Thus in May 1796 the Council of Five Hundred ruled that "the interests of society and morality" excluded women from senior teaching positions. *Moniteur*, May 11, 1796, vol. 28, p. 270.

vention's education committee both flirted with the idea of training women to set type, but nothing came of it.⁵⁶ Nor did the revolutionary governments make any effort to help the women injured by the collapse of luxury trades like silk and lace.⁵⁷ The government established *ateliers nationaux* for men quite early in the Revolution, but it was reluctant to help women. Jean-Sylvain Bailly, the mayor of Paris, requested aid for them in January 1790, but it was almost two years before anything was done, and then action came from municipal, not national, authorities. Where women were admitted to the *ateliers* they were regularly paid less than men.⁵⁸ In the Salpêtrière the administration relied in part on the profits of the unpaid labor of young girls to make ends meet.⁵⁹ Small wonder women continued to complain.⁶⁰

Under the Old Regime women could sometimes vote and act as regents; during the Revolution they assumed their right to form political associations. Less than five years after the calling of the Estates-General this had all disappeared. The legislators barely considered female suffrage despite the heated arguments the feminists had put forward. Abbé Emmanuel-Joseph Sieyès voiced the general opinion as early as July 1789. "Women, at least as things now stand, children, foreigners, in short those who contribute nothing to the public establishment, should have no direct influence on the government."⁶¹ The systematization of French electoral law eliminated the idiosyncrasies that had permitted women to vote; for the first time in centuries women were completely barred, as a group, from this aspect of the political process. Few people protested this exclusion. The women of Droits de l'Homme in Paris and the Républicaines Révolutionnaires castigated the provisions of the Constitution of 1793, but only by making speeches in the latter's club.⁶² Possibly the infrequency with which elections were held took the sting out of this exclusion; certainly at the level where politics really mattered, in the clubs and sections, women continued to vote for a time. Probably exclusion from the regency also mattered little, particularly when everyone was soon excluded by the abolition of the monarchy. The regency was not an important issue in itself, but it shows the ease with which the legislators could dismiss the idea of women participating in government.⁶³

⁵⁶ "Un arrêté féministe du Comité de salut public en l'an III," *Révolution française*, 60 (1911): 266; the meetings of Dec. 9, May 28, 30, 1793, and Aug. 20, 1794, in *Procès-verbaux du Comité d'instruction publique de la Convention nationale*, 3: 87; 4: 493, 503, 963.

⁵⁷ Hufton, "Women in Revolution," 96; Georges Duval, *Souvenirs thermidoriens* (Paris, 1844), 1: 53 n.2. Sullerot estimates that there were over 100,000 lacemakers in the region of Le Puy in 1789. *Histoire et sociologie du travail féminin*, 71.

⁵⁸ Bouvier, *Les femmes pendant la Révolution*, 156-58.

⁵⁹ Duc de la Rochefoucauld-Liancourt, "Rapport des Visites faites dans divers hôpitaux," 1790, in *Procès-verbaux et rapports du comité de mendicité*, 618.

⁶⁰ Report of Latour-Lamontagne, Sept. 13, 1793, in Caron, *Paris pendant la Terreur*, 1: 88.

⁶¹ Sieyès's preliminary remarks on the constitution, July 20-21, 1789, *Recueil des pièces authentiques approuvées par l'Assemblée nationale de France* (Geneva, 1789), 1: 193-99.

⁶² Duhet, *Les femmes et la Révolution*, 136-37.

⁶³ *AP*, Mar. 22, 23, 1791, vol. 24, pp. 261-67, 305-07.

Far more important to ordinary women than the vote or the regency was the issue of citizenship. Were women citizens enough to take the civic oath, one of the central means of demonstrating acceptance of the revolutionary ideals and of participating in communal life? In 1790, when the National Assembly swore the oath, the spectators, men and women, joined them.⁶⁴ Within two months women's right to take the oath had become an issue. Brigent Baudouin, wife of a municipal officer in Lanion, wrote the Assembly on behalf of several women in her village. "There is not a word about women in the Constitution, and I admit that they can take no part in government; nevertheless mothers can and should be citizens." They should therefore, she continued, be permitted to swear the revolutionary oath before the municipal officers. Goupil de Prefeln, a member of the Cercle Social, moved that all married women of "respectable conduct" be granted this honor. He added that mothers undoubtedly had more right to it than did childless women. The motion was tabled.⁶⁵ Swearing civic oaths became particularly important in the summer of 1790 during the Fêtes de la Fédération. In Beaune the National Guard invited eighty-four women to the ceremony, but the municipal authorities firmly refused to let them take part.⁶⁶ In Toulouse the city officials, momentarily forgetting *la galanterie française*, turned the fire hoses on the women present to disperse them.⁶⁷ Examples could be found of women who did take the oath and who were invited to sign petitions and make other symbolic gestures—for example, at the Champs de Mars in 1791—but the whole issue of women's citizenship remained clouded. With no sure rule to which to appeal, women had to depend on the good will of local authorities. Even at this low, but symbolically vital, level women's political status continued to be a matter of privileges not rights. The Committee of Public Safety and the Directory would both find themselves dealing with the consequences. A representative on mission, J.-B. Jérôme Bô, wrote the committee in 1794 to advise exemplary punishment of troublemakers, especially of those women who claimed that the law could not touch them because they had not taken the civic oath. In 1796 some factions used women to create disorder; since the government did not take them seriously, women could get away with subversive speeches for which men could be jailed.⁶⁸

The revolutionary governments had at one time taken women's activities quite seriously, but only long enough to outlaw their clubs. Apparently article 7 of the second Declaration of Rights, guaranteeing the rights of free speech and assembly, no more applied to women than did article 5, which

⁶⁴ *Ibid.*, Feb. 4, 1790, vol. 11, p. 432.

⁶⁵ *Ibid.*, Mar. 29, 1790, vol. 12, pp. 402-03.

⁶⁶ *Moniteur*, July 28, 1790, vol. 5, p. 240.

⁶⁷ *Révolutions de Paris*, Feb. 26-Mar. 5, 1791, no. 86, pp. 385-86n.

⁶⁸ Report of Mar. 29, 1794, in *Recueil des Actes du Comité de salut public*, ed. Alphonse Aulard (Paris 1889-1951), 12: 272; on the events of Apr. 17, 1796, see the *Courrier républicain*, quoted in Alphonse Aulard, *Paris pendant la réaction thermidorienne et sous le Directoire* (Paris, 1898-1902), 3: 126.

promised equal access to public office for all citizens. The Mountain sent the women's clubs crashing down in the fall of 1793. The ostensible cause was the unrevolutionary conduct of the *Républicaines Révolutionnaires*, a charge that could be supported in fact by the admiration of Claire Lacombe and Pauline Léon for Jacques Roux, Théophile Leclerc, and the *enragés*. The campaign against the *Républicaines* began in the Jacobin club on a strictly political note. A member announced that the women had taken up with Leclerc; François Chabot, Claude Basire, and Taschereau spoke against Lacombe's new political line. "I do not doubt that she is a tool of the counterrevolution," said Chabot sagely.⁶⁹ At this point the campaign against the *Républicaines* was specifically political and focused on them alone. Yet how soon the campaign changed! A month later a deputation of women from several sections came to the Convention to protest the activities of the *Républicaines*; one of them requested the abolition of their club. The Convention forwarded their complaint to the Committee of General Security. Fabre d'Eglantine made good use of this opportunity to address the Convention. After the *bonnet rouge*, which the *Républicaines* wore during their meetings, comes the gun belt, then the gun, he warned. He reminded the Convention of the manner in which the women went after bread: like pigs at a trough. These were not good mothers and daughters but—significant although false characterization—"des filles émancipées, des grenadiers femelles." The members several times interrupted his speech with applause. A little later one of the women spectators came forward to demand the abolition of all women's clubs.⁷⁰

The Convention must have been gratified by the report André Amar soon presented on behalf of the Committee of General Security. That committee, explained Amar, had considered two questions: should women exercise political rights and take part in government, and should women meet in political associations? From the specific case of the *Républicaines Révolutionnaires* the government had moved to consider the status of all French women. To both questions the committee replied in the negative. Women did not have the strength of character needed to govern; political meetings took them away from "the more important concerns to which nature calls them." Nature's imperious commands were not to be violated; women could have no political rights. Amar concluded:

There is another aspect of women's associations that seems dangerous. If we take into account the fact that the political education of men is still at its very beginnings, that all the principles are not yet developed, and that we still stammer over the word "liberty," then how much less enlightened are women, whose moral

⁶⁹ Meeting of Sept. 16, 1793, in *La Société des Jacobins: Recueil de documents*, ed. Alphonse Aulard (Paris, 1889-97), 5: 406-08. The Jacobins had earlier encouraged the *Républicaines* and granted them "affiliation and correspondence." Meetings of May 12 and Aug. 15, 1793, in *ibid.*, 356. There is little doubt that it was the evolution of the women's politics to the left which drew the Jacobins' fire.

⁷⁰ *AP*, Oct. 29, 1793, vol. 78, pp. 20-22.

education has been practically nonexistent. Their presence in the *sociétés populaires*, then, would give an active part in government to persons exposed to error and seduction even more than are men. And, let us add that women, by their constitution, are open to an exaltation which could be ominous in public life. The interests of the state would soon be sacrificed to all the kinds of disruption and disorder that hysteria can produce.

Impressed, the Convention quickly voted to outlaw all women's clubs.⁷¹

Had the government been content to close the Club des Citoyennes Républicaines Révolutionnaires without making these explanations its attitude would remain ambiguous. By expanding its target to include all women, of whatever political or apolitical stripe, the Committee of General Security and the Convention made it clear that political questions were merely a pretext. What they wanted to do was to exclude women, as a group, from public life. Anaxagoras Chaumette, the *procureur* of the Paris Commune, summed up the new order a fortnight later. Speaking in response to the arrival of a deputation of women at the Conseil Général of the Commune he lectured: "So! Since when have people been allowed to renounce their sex? Since when has it been acceptable to see women abandon the *pious* duties of their households, their children's cradles, to appear in public, to take the floor and to make speeches, to come before the senate?"⁷² The Committee of Public Safety drove the message home in an "Avis aux Républicaines," which appeared in the semiofficial *Feuille du Salut Public*. The committee began its admonition on a menacing note. It reminded women of the fate of Marie-Antoinette, de Gouges, and Mme Roland. The purpose of this reminder was strikingly clear in the choice of de Gouges. "She wished to be a politician and it seems that the law has punished this conspirator for forgetting the virtues appropriate to her sex"—that is, not for the character of her opinions but for having had opinions. This lecture concluded by spelling out the virtuous life.

Women! Do you want to be Republicans? . . . Be simple in your dress, hard-working in your homes, never go to the popular assemblies wanting to speak there. But let your occasional presence there encourage your children. Then *la Patrie* will bless you, for you will have done for it what it has a right to expect from you.⁷³

Few people protested the suppression of the women's clubs. Lacombe brought a deputation of women to the Convention the following day; the deputies howled them down and hooted them out.⁷⁴ In the provinces the

⁷¹ *Moniteur*, Oct. 30, 1793, vol. 18, pp. 299–300. One member alone objected. He was ignored. Note that the decree against the women's clubs was a first step to the suppression of the *sociétés populaires*.

⁷² *Ibid.*, Nov. 17, 1793, vol. 18, p. 450, italics in original.

⁷³ *Ibid.*

⁷⁴ *AP*, Nov. 5, 1793, vol. 78, p. 364. Caron, *Paris pendant la Terreur*, in which one might expect to find indications of popular reaction to the Convention's decision, is particularly fragmentary for October–November 1793. Possibly there was a good deal of protest that has left no trace, but this is not probable.

clubs quietly dissolved. For a time the women in Paris could continue to participate in sectional assemblies and mixed clubs like the Société Fraternelle du Panthéon.⁷⁵ Perhaps this softened the blow; in the capital at least women still had political outlets. Yet their status in the men's clubs was unclear. In the assembly of the Panthéon-français section a deputation from the Société des Amis de la République was warmly applauded when its spokesman asserted that the ban on women's clubs also forbade them to vote in other clubs. A member of the Paris Commune disagreed, and the matter was dropped.⁷⁶ Vague reports of women's organizations crop up in records from later periods—a leader of a “club des femmes jacobites” was arrested in May 1795; earlier the police had flushed out a “nid des jacobines”—but these reports are too ambiguous to prove anything about women's political activities.⁷⁷

The suppression of the women's clubs effectively destroyed the feminists' political aspirations. It was not, however, the clearest statement on women's rights the government made. After the *journée* of 1^{er} Prairial of the Year III (May 20, 1795), the Convention voted to exclude women from its meetings; in future they would be allowed to watch only if they were accompanied by a man carrying a citizen's card.⁷⁸ Three days later the Convention placed all Parisian women under a kind of house arrest. “All women are to return to their domiciles until otherwise ordered. Those found on the streets in groups of more than five one hour after the posting of this order will be dispersed by force and then held under arrest until public tranquillity is restored in Paris.”⁷⁹ The progress of the Revolution had rendered the brave hopes of the feminists of 1789–91 chimeric.

Only in regard to their legal status could feminists find some gratification. The Revolution, so severe to women in public life, was kinder to them in private life. Inheritance laws were changed to guarantee male and female children equal rights.⁸⁰ Women reached majority at twenty-one under the new laws. Moreover they could contract debts and be witnesses in civil acts.⁸¹ Other legislation changed the laws concerning women's property, giving them some voice in its administration, and acknowledged the mother's part in decisions affecting her children.⁸² Revolutionary divorce

⁷⁵ See the reports in Caron, *Paris pendant la Terreur*, Dec. 29, 1793, vol. 2, p. 75; Jan. 7, 1794, vol. 2, p. 227; Jan. 16, 1794, vol. 2, p. 398; Jan. 25, 1794, vol. 3, p. 149; Jan. 27, 1794, vol. 3, p. 170; Feb. 5, 1794, vol. 3, p. 342; Mar. 26, 1794, vol. 6, pp. 117–18; Mar. 30, 1794, vol. 6, p. 207. March 30 is the last date for which Caron gives reports.

⁷⁶ *Ibid.*, Feb. 14, 1794, vol. 4, pp. 112–14.

⁷⁷ Police report for May 26, 1795, and excerpt from the *Courrier républicain* of Jan. 23, 1795, both quoted in Aulard, *Paris pendant la réaction thermidorienne*, 1: 748, 421.

⁷⁸ *Moniteur*, May 20, 1795, vol. 24, p. 515.

⁷⁹ Meeting of May 23, 1795, in *Procès-verbal de la Convention nationale* (Paris, 1792—an IV), 62: 67. I have found no trace of the repeal of this unenforceable law.

⁸⁰ Felix Ponteil, *Les institutions de la France de 1814 à 1870* (Paris, 1966), 170.

⁸¹ *AP*, Sept. 20, 1792, vol. 50, p. 181; Godechot, *Les institutions de la France*, 48. Despite the new law the Ministry of the Interior was still in doubt as to women's rights in respect of civil acts a year and a half later. *AP*, Jan. 14, 1794, vol. 83, p. 338.

⁸² Garaud, *La Révolution et l'égalité civile*, 178; Sagnac, *La législation civile*, 296; Sullerot, *Histoire de la presse féminine*, 65.

legislation treated both sexes equally.⁸³ Yet some inequalities remained. Women could not serve on juries; in practice they were excluded from sitting on the Tribunaux de Famille, which attempted to settle family quarrels from 1790 to 1796.⁸⁴ Moreover the gains were short-lived. The Napoleonic codes swept away almost every advance the women had made, returning them to the status Pothier had described in 1769.⁸⁵

THE MOST IMPORTANT REASON for the almost total failure of revolutionary feminism was its narrow base. Feminism was and remained a minority interest. The majority of French women—the logical constituency to which the movement could hope to appeal—had no interest in changing their social position. For the most part French women accepted the eighteenth-century definition of femininity. Far more typical of their attitudes than any of the feminist manifestoes is this speech made by the women of Épinal to their men.

If our strength had equaled our courage we would, like you, have hastened to take up weapons and would have shared with you the glory of having won our freedom. But it took stronger arms than ours to defeat the enemies of the Constitution; our weakness has prevented us from taking part in this Revolution. We content ourselves with admiring your efforts.⁸⁶

The feminist movement had been unable to reach these women. Neither its words nor its action had made any sense to ordinary women. Feminism never became part of the program of the majority of the women's clubs. Only the Besançon club considered urging the Convention to extend the suffrage to women, but faced with the mockery of local Jacobins it soon abandoned the project.⁸⁷ At Orléans feminism never raised its head.⁸⁸ One of the few lengthy series of *procès-verbaux* available from a women's club, that of Ruffec (Charente), shows not a hint of feminist attitudes in two years.⁸⁹ The women's clubs were content to function as auxiliaries to male societies. The mixed clubs held themselves equally aloof, except for the short-lived efforts of the Cercle Social. If the various *sociétés fraternelles des deux sexes* approved of feminism, they kept the secret to themselves.

The prominent women of the Revolution are conspicuous by their ab-

⁸³ On the legislation of Sept. 20, 1792, see *Moniteur*, Oct. 10, 1792, vol. 14, pp. 158–60.

⁸⁴ J. Forcioli, *Une institution révolutionnaire: Le Tribunal de Famille d'après les archives du district de Caen* (Caen, 1932), especially p. 39. See also *Moniteur*, Aug. 1, 1791, vol. 9, p. 276.

⁸⁵ The spirit of the civil code is summed up in its article 213: "La femme doit obéissance au mari." One of the few gains women preserved from the revolutionary legislation was the principle of equal inheritance.

⁸⁶ "Compliment fait par les citoyennes d'Épinal à MM. le Députés arrivant de la Confédération générale de Paris, le mercredi 28 juillet 1790," *La Révolution dans les Vosges*, 17 (1929): 47.

⁸⁷ Meetings of Feb. 6, 1793, and after, according to Langeron, "Le club des femmes de Dijon," 6.

⁸⁸ Bloch, "Les femmes d'Orléans," 67.

⁸⁹ Chauvet, "Registre de la Société des amies des vrais amis de la Constitution à Ruffec (Charente), 1791–1792," *Révolution française*, 46 (1904): 246–78.

sence. Mme Robert, coeditor of the *Mercure national*, belonged to the Société Fraternelle des Jacobins, but she was no feminist. She told her club, apropos of women inspectors for the public hospitals, that women could contribute greatly to the success of the inspections, but she went on to add, "Their domestic duties, sacred duties important to the public order, prohibit their taking on any administrative functions, and I do not claim to draw them from their sphere."⁹⁰ Mme Roland, too, accepted the status quo. "I am often annoyed to see women arguing over privileges that do not suit them; even the title of 'author' seems ridiculous for a woman to me. However gifted they may be in these fields, they ought not to display their talents to the public."⁹¹ The Directory is often described as a woman-dominated regime and Mme Tallien cited as a leading example of women's power in this era. However she once wrote to the Convention, "Woe indeed to those women who, scorning the glorious destiny to which they are called, express, in order to free themselves of their duties, the absurd ambition to take over men's responsibilities."⁹² Mme de Staël, perhaps the most important of the revolutionary women, seems to have had some feminist leanings, but she certainly cannot be brought forward as an activist.⁹³ The pattern is clear: the most famous women of the period were careful to give the disreputable feminists a wide berth.

Nor did the supporters of women's rights capture the backing of the leading men of the Revolution. Condorcet was a real anomaly. Far more typical was Mirabeau, who gushed over the "irresistible power of weakness," warned that women's delicate constitutions limited them to the "shy labors" of the home, and pondered whether they should ever be let out of the house.⁹⁴ Jacques-René Hébert, as one would expect, did not gush. Although he took some earthy shots at wife beaters—"ces bougres de tyrans"—his sympathies were limited.⁹⁵ Robespierre's attitude remains enigmatic. Jacques Godechot asserts that he spoke in favor of votes for women in the Constituent Assembly, but other commentators place him in the opposite camp. The volumes of his *Oeuvres complètes* published to date shed no light.⁹⁶ Louis-Antoine Saint-Just would go as far as agreeing that laws on adultery should be equal for both sexes but, like Mirabeau, he belonged to the "faiblesse intéressante" school and urged that girls be educated at home,

⁹⁰ Quoted in Villiers, *Histoire des clubs de femmes*, 50–51.

⁹¹ Quoted in L. J. Larcher and P. J. Martin, *Les femmes peintes par elles-mêmes* (Brussels, 1858), 68–69.

⁹² *AP*, Apr. 23, 1794, vol. 89, p. 215.

⁹³ See Madelyn Gutwirth, "Mme de Staël, Rousseau and the Woman Question," *Publications of the Modern Language Association*, 86 (1971): 100–09.

⁹⁴ *AP*, Sept. 10, 1791, vol. 30, pp. 518–19.

⁹⁵ Jacques-René Hébert, *Le Père Duchesne*, Dec. 6, 1790, no. 31, in *Le Père Duchesne d'Hébert*, ed. Fritz Braesch (Paris, 1938), 391.

⁹⁶ Godechot, *Les institutions de la France*, 47. I have been unable to trace this motion. Sullerot (*Histoire de la presse féminine*, 63), Villiers (*Histoire des clubs de femmes*, 248), and Lytle ("Second Sex," 23) all consider Robespierre to be an opponent of women's emancipation.

with due regard for the preservation of their chastity.⁹⁷ For the rest, we can find clues in their newspapers. Louis Prudhomme's *Révolutions de Paris* reveled in misogyny.⁹⁸ Jean-Paul Marat, Camille Desmoulins, and Gracchus Babeuf ignored the women's movement.⁹⁹ Jacques Roux, like Condorcet, was an exception, but as a defender he was hardly an unmitigated blessing.¹⁰⁰ The feminists, then, had been unable to win the backing of any of the important Revolutionary factions. Their following was confined to a few clubs and to isolated individuals, many of them political moderates whom the progress of the Revolution incidentally eliminated.

The characters of the feminist leaders were scarcely the sort to find favor with the respectable. Of those whose lives we know, only Condorcet was above reproach. The pretensions to gentility of de Gouges, Théroigne de Méricourt, and the "Baronne" d'Aelders struck contemporaries as ludicrous, and this amusement carried over to their activities. The unsavory histories of Théroigne de Méricourt and Claire Lacombe did not help the movement any more than did Lacombe's and Léon's liaisons with the *enragé* Leclerc. While male revolutionaries might be forgiven their sexual peccadilloes, the women could count on no such toleration. Even as they protested the existence of a double standard it was at work against them. All of the feminist leaders were further compromised by their political convictions, whether moderate or extremist. Moreover the feminists were all held guilty for the acts of all other women—the emigrés, the *tricoteuses*, Marie-Antoinette, Charlotte Corday. Protest as they might, the feminists could never convince the public that the principle of collective responsibility should not be applied to the whole sex.

The feminists made tactical and strategic errors. Women's groups allowed themselves to be distracted too easily. The Républicaines Révolutionnaires let themselves become embroiled in street fights over the wearing of the *cocarde* and the *bonnet rouge*.¹⁰¹ All of the women's clubs suffered from their habit of putting other people's causes before their own. The provincial

⁹⁷ Louis-Antoine Saint-Just, "Esprit de la révolution et de la constitution française" (1791), in *Oeuvres complètes de Saint-Just*, ed. Charles Vellay (Paris, 1908), 1: 291-92; Saint-Just, "Fragments sur les institutions républicaines" (1793-94), in *ibid.*, 2: 519.

⁹⁸ For example, "De l'influence de la révolution sur les femmes," *Révolutions de Paris*, Feb. 5-12, 1791, no. 83, pp. 226-35; "Addition à l'article des femmes pétitionnaires, no. 124," *ibid.*, Dec. 10-17, no. 127, pp. 497-500; "Club de femmes à Lyon," *ibid.*, Jan. 19-26, 1793, no. 185, pp. 234-35; "Femmes contre-révolutionnaires en bonnet rouge," *ibid.*, Nov. 4, 1793, no. 213, pp. 150-51. Prudhomme demonstrated his "sympathy" for the Républicaines by publishing, after their dissolution, a *procès-verbal* of one of their last, and chaotic, meetings, that of October 28, 1793. "Procès-verbal de ce qui est arrivé aux Citoyennes républicaines-Révolutionnaires," *ibid.*, Nov. 13-20, 1793, no. 215, pp. 207-10.

⁹⁹ See their journals, respectively, *L'Ami du peuple* and the *Journal de la République française*; *Révolutions de France et de Brabant* and the *Vieux Cordelier*; and *Tribun du peuple*.

¹⁰⁰ Jacques Roux, speaking to the Paris Commune, June 21, 1793, in *Scripta et Acta*, ed. Walter Markov (Berlin, 1969), no. 51, pp. 472-73.

¹⁰¹ The wearing of the *cocarde* and the brawls this caused gave the government another pretext to move against the women's clubs. The affair is complicated and is more easily followed in Duval, *Souvenirs thermidoriens*, 1: 52-54, than by tracing it through either the *AP* or the *Moniteur*.

clubs settled meekly into ladies' aid societies, and even the fiery Républicaines were more interested in the price of bread than in women's wages. However commendable these positions may have been as expressions of largeness of spirit, they were sorely damaging to any attempt to work specific, radical change. The feminists showed other signs of political and managerial inexperience. They acted in isolation: individual leaders had no verifiable contacts with each other; the clubs proceeded independently, and the occasional attempts to set up a national organization came to nothing.

It would seem, too, that that vague entity, the spirit of the times, ran counter to the feminist revolution. One important aspect of this counter-current was the ideal of the nuclear family. Time and again feminists tripped over the conviction that the changes they advocated were unnatural because women belonged in the home. This was the most frequent explanation given for refusing their requests. The idea of the family as a secure nest, maintained by the wife, to which the husband retired from his toil in the outside world, was a relatively recent development. It certainly did not reflect the reality of lower-class life, for lower-class women could not afford to spend all their time keeping house. It was the wealthy who developed a hagiographic tradition around the family.¹⁰² Once women were firmly confined to the home there was no "need" for feminism, and the majority of middle-class politicians could only gaze upon it in blank astonishment. To their way of thinking, refusing the feminists' demands ought to have been counted as so many acts of kindness toward women, who were by nature too delicate for the dirty world into which the feminists tried to thrust them.

Revolutionary feminism began in a burst of enthusiasm. Its unpopularity, its own mistakes, and the blissful incomprehension and dogmatism of its opponents combined to obliterate it. While it lasted it was a very real phenomenon with a comprehensive program for social change, perhaps the most far-reaching such program of the Revolution. This very radicalism ensured that it would remain a minority movement, almost the preserve of crackpots. Influential contemporaries turned out speech after speech, newspaper after newspaper, report after report without ever acknowledging its existence. Despite its minority nature and its abject failure, revolutionary feminism is not without significance. It illustrates, as clearly as anything can, the changing seasons of the Revolutionary calendar and stands as striking proof of the essential social conservatism of this political upheaval.

¹⁰² Bardèche, *Histoire des femmes*, 2: 193. For examples of worshipful attitudes toward the family, read Edmond Pilon, *La vie de famille au 18^e siècle* (Paris, 1923) and almost any of the proposals for national education listed in note 54 above. This ideal filtered down to the sans-culottes. See Albert Soboul, *The Parisian Sans-Culottes and the French Revolution, 1793-1794* (Oxford, 1964), 244-45, but note the qualifications on common-law unions on pages 245-46. See also Duhet, *Les femmes et la Révolution*, 139. The convention of the idle wife may have originated with the aristocracy, but the idleness of aristocratic women is not comparable to that of bourgeois women. Aristocratic men could also make a virtue of inactivity, while bourgeois men are usually seen as making a cult of activity.

Reviews of Books

GENERAL

WILLIAM TODD. *History as Applied Science: A Philosophical Study*. Detroit: Wayne State University Press. 1972. Pp. 250. \$11.95.

WESLEY MORRIS. *Toward a New Historicism*. Princeton: Princeton University Press. 1972. Pp. xii, 265. \$10.00.

Is history applied science? William Todd, who teaches philosophy at the University of Cincinnati, argues that it can be. In "Types of History," the longer introductory section of his work, he presents a dozen specific historical problems drawn from as many historians, then in the brief concluding argument, "A Methodology for History," attempts to convince the reader of his thesis. Because in their treatment of a given problem historians such as Huizinga, Brinton, Granet, Bullock, Namier, and others speculate that circumstances and choices might have been different, Todd concludes that all historians employ assumptions contrary to fact (counterfactuals) in order to highlight significances and to pronounce ethical judgments on the actors of the past.

Todd's argument for history as applied science hinges on his assertion that games, models, and operations research can be devised to provide dynamic simulations of actual historical situations. Would it not be possible, he suggests, to build a model that would simulate the past, then "simply plug in the constants which represent the past situation, . . . and play the simulation out a large number of times to see if the average outcomes are the ones that actually occurred" (p. 198)?

What would be the purpose of such an enterprise? "The practical suggestion here is that historical understanding can be perfected and can be carried further than it usually is if we do resort to the setting up and playing of dynamic simulations"; moreover, the methodology

"forces us to make a more complete historical investigation than would have been made without the guidance of the simulation" (pp. 237-38). Yet all this extra effort may not be worthwhile if Todd is in error in his basic assumption that scientific causality and historical causality are alike. It would appear impossible to feed into the computer-operated simulation the dreams, hopes, and fears of the neurosis that is man. Even if repeated playings with randomly selected variables offered a pattern of probabilities it is well to recall the observation of Teilhard de Chardin: "In terms of physics and chemistry, the phenomena of life are essentially characterized (in precise contrast to those of matter) by an evolution toward the *least probable*." Is it otherwise with human history?

Wesley Morris of Rice University takes us on an exploratory journey into the frontiers of the new historicism, a term he employs to describe a now emerging form of American literary criticism. In unrolling the map of historicism he orients the observer quickly. Historicism is essentially a search to uncover a meaningful continuity in history by seeking for principles in the product of men's minds that will provide meaningful relationships. Transcending his own present the historicist enters into the minds of the past and builds a continuity between past and present.

Traditional historicism in literary evaluation Morris classifies according to four types: "metaphysical," which intuitively perceives a transcendent universal; "naturalistic," which borrows its principles from the natural and social sciences; "nationalistic," which uses the political or racial unit as the framework for its meaning; and "aesthetic," which focuses on the creative act of the novelist or poet. The first three tend to be aesthetically blind as meaning becomes confined to the extrinsic context of historical process. Aesthetic historicism, however, tends to collapse the literary work and the observed

historical context into the mind of the creative writer.

Morris argues that true criticism must deal with both aesthetics and historical interpretation. For literature exhibits a dual character: it is at once embedded in the time period of its creation, mirroring that time, and simultaneously a monument that rises above its circumstance to command attention on its own aesthetic merit. The new historicism essays the view that literature "is at once in history and above it," in the words of Roy Harvey Pearce, whose criticism has emerged from the traditional historicist side. Morris finds the fullest development of the new historicism in the aesthetics of Murray Krieger, who sees in the unique structure and language of a poem both the timeless aesthetic world and the existential, nonpoetic world of the poet.

While Morris addresses his work to students of American literature his excursion may be profitably traced by the student of American intellectual history or indeed by any historian who wishes to renew his understanding of historicism. The study is not a chronological survey but is problem oriented, and although the preface warns that it is "narrowly philosophic in nature," the reader is never at a loss as to his whereabouts and is carried forward by the compelling excitement of discovery.

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PARDON E. TILLINGHAST. *The Specious Past: Historians and Others*. (Addison-Wesley Series in History.) Reading, Mass.: Addison-Wesley Publishing Company. 1972. Pp. vii, 198. \$2.75.

This book "is concerned with why some people have troubled to spend their lives writing history and why others spend their leisure . . . in reading what the historians write" (p. iii). It contains a discursive examination of the purpose of the discipline, discussions of what historians and others have said about that purpose, and a defense of the present utility of studying the past. Tillinghast is particularly interested in the general reader, sympathizing with his desire to have historians tell meaningful, true stories of broad significance.

It is a difficult book to classify. As Tillinghast says, it "has the nature of an inquiry rather than a narrative or analysis" (p. 170). After a learned, but brief, account of "the relationship between historians and their audience" (p. iv) he proceeds to a consideration of the relevance

to history of science, literature, philosophy, and theology. Next there is a chapter about "historical rebels": Nietzsche, Spengler, Toynbee, and the New Left, presented as writers who have tried to recall historians to a broader conception of their role. Historicism is examined as an attempt on the part of some historians to defend the autonomy of the discipline. A relatively lengthy discussion of the role of moral judgments in history argues strongly that historians do and ought to make them.

There are some problems with this serious and engaging book. Its presentation of historical thought is extremely condensed, but its organization leads to some repetition. Tillinghast maintains that historians can and should provide their own time with a usable past, but the strategy of his study does not serve that argument consistently. He describes present issues in the discipline largely in terms of the history of similar issues in the past, drawing our attention away from the distinctive features of the contemporary situation. Perhaps it is unfair to criticize so short a book on so large a subject for omissions, but it is surprising to find no examination of Marx in a study concerned with the utility of history, particularly one that deals with historical rebels.

I have no quarrel with Tillinghast's conclusion, a fine humanistic defense of the continuing importance of the writing of history as offering "a deeper and fuller understanding of what we can and cannot be" (p. 178).

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RADOSLAV A. TSANOFF. *Civilization and Progress*. [Lexington:] University Press of Kentucky. 1971. Pp. vi, 376. \$12.50.

W. WARREN WAGAR, editor. *History and the Idea of Mankind*. (Sponsored by the Council for the Study of Mankind.) Albuquerque: University of New Mexico Press. 1971. Pp. xv, 227. \$12.00.

To provide a framework within which to discuss these two books of 1971, it is useful to turn to Thomas Kuhn's book of 1961, *The Structure of Scientific Revolutions*. Here Kuhn challenged the idea that modern science represented progress away from premodern scientific communities. Modern historians of science, according to Kuhn, postulated that modern scientists were able to make progress toward understanding an ultimate reality because they approached reality as individuals while premodern scientists were kept from reality by the

perspectives of their cultures. Kuhn argued, however, that modern science, like premodern science, depended upon hypotheses that expressed a cultural definition of reality. Modern scientists, like premodern scientists, worked as members of a community built on the acceptance of what Kuhn called paradigms.

If one applies Kuhn's model to the assumptions of modern historians in other areas such as politics or economics, one finds a challenge to the whole idea of modern history as the history of liberty, as the liberation of the individual from cultural boundaries. Kuhn's model, of course, is in accord with twentieth-century anthropologists who define man as a cultural animal. Kuhn insists that one scientific culture cannot be judged as inferior or superior to a different scientific culture. This relativism is central to the logic of the twentieth-century anthropologists who can no longer define primitive as inferior to civilized, who indeed can no longer define what is primitive.

Many intellectuals, however, seem to be working with the paradigm of nineteenth-century anthropology, which believed there was an inevitable progress from the savage childhood of mankind, through barbarism to a final civilized maturity. The philosopher Radoslav Tsanoff, in his book *Civilization and Progress*, accepts this series of stages. His book traces the history of the idea of progress from its appearance in the seventeenth century to its challenge by nineteenth- and twentieth-century intellectuals. Tsanoff rejects this criticism because he sees it based on the assumption that progress is inevitable. But, according to Tsanoff, when man has reached the stage of civilization, he then has increased choices for both good or evil. For Tsanoff then, human progress is unambiguous when it moves from savagery to civilization because that is when the individual is liberated from group definitions. Once the individual becomes free in the stage of civilization, he can make the choice of good or evil.

History and the Idea of Mankind represents a symposium organized by the Council for the Study of Mankind, an association of scholars founded in 1952 who were concerned that "the material unity which already and irrevocably exists [in the world] must be reinforced by legal, moral, and spiritual unity, which sadly . . . still does not exist."

It was hoped that this symposium would further the historical consciousness necessary for world unity. Part 1 includes essays on the idea of mankind held by five traditional societies.

Kees Bolle discusses the idea in premodern India; Helmut Callis discusses China; Eliezer Berkovits discusses Judaism; S. D. Goitein discusses Islam; and W. Warren Wagar discusses classical and medieval Europe. Part 2 has essays by Hans Kohn on "Nationism and Internationalism," Melvin Kranzberg on "Science, Technology and the Unity of Mankind," Robert Lystad on "Race: Unity in Diversity," and W. Warren Wagar, "Religion, Ideology, and the Idea of Mankind in Contemporary History."

The book is a fascinating document that illustrates the increasing competition of paradigms within the historical profession. Bolle and Callis write with great respect for the premodern Indian and Chinese theories of unity within diversity. The logic of their position is similar to the relativism of Kuhn and the twentieth-century anthropologists. In no way do they write as if these premodern perspectives are more childish or less realistic than the social theories of the modern West.

On the other hand, Melvin Kranzberg imputes progressive values to modern science, which Kuhn rejects. Kranzberg sees modern science ending the darkness of the medieval world: "The idea of inexorable law operating in the political, social, and economic sphere in order to secure human happiness was a liberating idea which freed men from the grasp of superstition," and "the demands of an advancing technology will inevitably require equality of social, educational, and economic opportunity for the Negro population of the United States."

The most interesting expression of the paradigm crisis among current historians is the concluding essay by W. Warren Wagar. Writing as a historian of ideas, he describes a crisis in the modern belief in progress: "From the middle of the 18th century down to the middle of the 20th . . . the central project of the Western spirit was to find a world-encompassing faith to replace traditional Judaism and Christianity. . . . The great fact in the spiritual life of Western man in the years just after the 2nd World War was the apparent suspension of the search for new systems of secular faith. He suddenly found himself living in a post-ideological, anti-Utopia, demythologized world in which the will to believe had withered and failed." But Wagar dissociates this modern faith in progress from modern faith in science and technology and believes that "we must look forward to highly organized societies that will probably evolve in the direction of a unified world civilization. . . . It will be a world of science and

technique, of large-scale industry and government planning."

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E. J. HOBBSAWM *et al.* *Historical Studies Today*. Edited by FELIX GILBERT and STEPHEN R. GRAUBARD. New York: W. W. Norton and Company. 1972. Pp. xxi, 469. Cloth \$12.00, paper \$3.95.

This sizeable tome is a reprint of the essays in the winter and spring 1971 issues of *Daedalus*. Its twenty authors and two editors deal with the ramifications of two questions: Are traditional historical fields being studied in a new way, and have new fields appeared? The answer to the second question is clearly yes, and to the first, just as clearly, yes and no.

The fields covered are traditional (political, diplomatic, economic, social, military, and intellectual) and newer (quantitative, history of science, psychological, urban, prosopographical, and oral). Some essays do not fit into any particular category, such as Schlesinger's on the historian as participant rather than as memoirist, Dumont and Ladurie's on the quantitative study of French military archives, and Schwartz's on the relation between political and intellectual history in non-Western countries.

None of the essays is in any way incompetent, which is hardly surprising considering the high reputation of most of the authors; some of the essays are first-rate, especially those of Hobsbawm, Manuel, Le Goff, Schlesinger, and Vansina. Several of the positions are quite predictable, for instance, bows toward computerization, demography, the Annales school, and structuralism; the normal denigration of Whig history and the more old-fashioned, impressionistic political or intellectual history, especially that in Ranke's tradition; and the denial of space to any European period before the eighteenth century.

There is a distinct pecking order: social, quantitative, urban, and oral history and prosopography do not need defense at present, and are extending and consolidating their respective turfs. On the other hand the proponents of educational and intellectual history wish to tie them more firmly to theories of society as a whole. The tone of the political and diplomatic articles is noticeably defensive, and the essay on history of science offers an all too convincing proof that most historians have given at least its modern aspects a very wide berth.

This ranking seems to correspond closely to the present interests of the profession as a whole, or at least of its more voluble practitioners; that is, it is very sound. Fortunately, new ideas as well as prevailing orthodoxies keep surfacing. Some of the best sections are excursions inside the articles, for example, Furet on "histoire événementielle" and serial history; Stone on elitism versus statistically mass-minded history; Le Goff on the implications of "polis" and "urbs"; and Paret on the implications of psychological theory on the history of war. Vansina's whole chapter can be read as an allegory for the edification of American and European historians whose minds are fixed on immutable categories of what constitutes proper historical evidence.

The overall theme is that the best history now is "scientific." Since, in view of Kuhn's essay, this obviously does not mean science-oriented, it must mean that it is written chiefly for the guidance of other historians (and their apprentices). This is a pity because it suggests that nonspecialists are no longer an acceptable audience, except peripherally. It is also a pity that the editors have restricted their purview so firmly. In such a large volume, with space for excursions and technical case studies, it is surprising that no room was found for any historians not based in the United States (preferably Princeton), France, or England, and even more surprising that the relation of history to any of the humanities is not discussed at all. Had the book been entitled "History and the Social Sciences," this would have been understandable, but the implication is that either the editors felt no work was being done in, for example, philosophy of history, religious history, or the history of art or literature; or else that what there is in these fields is unworthy of attention. The former proposition is not true, so one is driven to the latter. No one objects to rigor in historical research, but does this rigor exist exclusively on the frontiers between history and social science? The issue is real, and the preface or introduction might have been a good place to have made the editors' reasoning clear.

The volume has been well edited, and it occupies a worthy place with the volumes by Higham-Krieger-Gilbert, Gottschalk on generalization, and the earlier SSRC bulletins on historiography. Like them, it is a monument to the new historical scholasticism.

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ALFRED W. CROSBY, JR. *The Columbian Exchange: Biological and Cultural Consequences of 1492*. Foreword by OTTO VON MERING. (Contributions in American Studies, number 2.) Westport, Conn.: Greenwood Publishing Company. 1972. Pp. xv. 268. \$9.50.

Alfred W. Crosby, Jr., in his short, compact book of six essays illuminates the idea that only rarely in the preindustrial age could man alter the delicate balance of nature. But when Columbus opened the Western Hemisphere to European immigration and settlement he initiated a chain of biological changes that were ultimately global in scope. The interchange of organisms that transformed New and Old World ecology is a complex subject. The author concentrates on only three important aspects of the exchange: the epidemic diseases that ravaged both hemispheres, especially smallpox, measles, and syphilis; transplantation of Old and New World food crops, particularly wheat and potatoes from the East and maize, beans, and manioc from the West; and the introduction of European farm animals into the New World. The opening chapter outlines the thesis, stressing that evolution produced specialized flora and fauna in the Western Hemisphere that differed from those of the Old World. A study of blood types points out the homogeneity of the New World's human inhabitants. Beginning with the Columbian voyages, destructive exchanges of microorganisms decimated humanity on both sides of the Atlantic. In the essay "Conquistador y Pestilencia" the well-known relationship between epidemic disease and the conquest of the Americas is briefly described, emphasizing the role of smallpox and measles. In a fast and flowing discussion of the introduction of plants and animals into the New World unintentional transfers are brought sharply into focus. An essay on syphilis opens by comparing the Columbian hypothesis with the Unitarian theory of the disease's origin. The author concludes that the Columbian thesis is still valid. Concerning the introduction of New World food products into the Old World the author demonstrates the connection between Columbus and the population explosion. In so doing he re-evaluates the Malthusian theory. In his final essay Professor Crosby points out that the Columbian exchange has continued down through the centuries to the present. He concludes that world ecology has been permanently altered as continuing combination and competition between life forms developed increased homogeneity. His final conclusion is pessimistic: "The Columbian exchange has left us with not a richer but a more

impoverished genetic pool. We, all of the life on this planet, are the less for Columbus, and the impoverishment will increase."

While much of the author's evidence comes from Latin America, especially the well-documented Spanish American colonies, he ranges widely for examples and illustrations from other areas. With a preponderance of material drawn from the Latin American experience the absence of information about early botanical surveys and useful drugs is disappointing. However, Professor Crosby's data is reliable, his text well written, and his ideas stimulating. He successfully synthesized a wide assortment of medical, cultural, and historical materials of great complexity. Unfortunately, there are a number of naive generalizations that are questionable. For example, the comment that in the fifteenth century "the Bible was the source of most wisdom" is an oversimplification. Most historians will not allow that "the most important changes brought on by the Columbus voyages were biological in nature." It is doubtful if anthropologists will accept: "No civilization has ever satisfactorily solved the problem of sex." Apart from such obvious statements the book is an excellent summary of an important subject.

EDWARD E. BARRY, JR.
Montana State University

RONALD H. CHILCOTE, editor. *Protest and Resistance in Angola and Brazil: Comparative Studies*. (Published under the auspices of the African Studies Center and the Latin American Studies Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xvi, 317. \$12.00.

The first European power to participate in transatlantic capitalistic imperialism was Portugal, and the resulting heritage of exploitation, enslavement, and oppression burdens Brazilians and Angolans still. In Africa colonial status itself is only now disappearing. In Brazil a system of personal relationships characterized by hierarchy, deference, and submission perpetuates the colonial experience. As an African journalist pointed out in 1901, the Portuguese bequeathed "an authoritarian and violent way of life."

The reaction of the victims over a period of five hundred years has been largely muted and ineffective. The essays gathered in this book—which resulted from a series of papers presented in 1968—demonstrate that the only movements of "protest" that have had any significant impact were either elitist or reactionary. Manuel

Correia de Andrade, for instance, describes the four-year revolt of the Cabanos in Brazil in the 1830s, which aimed at the restoration of an absolutist king. Similarly, Ralph della Cava narrates the collaboration of an early twentieth-century folk leader with the landed and mercantile oligarchs of the arid Brazilian Northeast.

Other movements studied here are deeply ambiguous in their significance. Two papers—by Alfredo Magarido and René Ribeiro—deal with religious movements, one in Africa and one in Brazil, and several others refer to messianic, sectarian, or church-directed social manifestations. But none of them deals systematically with the question alluded to by one author: Are these movements “the only possible form of resistance . . . [making] the exploited aware of their exploiters,” or have they rather diverted “the resistance of the exploited groups . . . [and] retarded the struggle?” Similarly, the rise of banditry may be seen as a form of protest in which the outlaw is a kind of Robin Hood, but he can also be interpreted, as Amaury de Souza seems to prefer in discussing the Brazilian case, as the result of the declining ability of landed patriarchs to control the private armies they had created. The concomitant result there was that the leaders turned to banditry for personal gain, creating an “entrepreneurship of violence.” For neither banditry or messianism is the social psychology of the followers explored here; concentrating attention on leaders, the essays leave the reader predominantly with the notion of the retrograde purposes of these social phenomena.

Those movements that seem to have been genuine expressions of protest and resistance were discouraging failures. Michael Samuels explores an inconsequential demand for better educational facilities in Angola in 1910. Douglas L. Wheeler details some of the protest writings of the Angolan *assimilados* before 1930 and comes to the sad conclusion that they discovered that if they “desired ‘progress’ in political rights . . . they would have to cooperate with the authorities by criticizing African rebellions.” Shepard Forman narrates the unsuccessful struggles of peasant leagues in northeast Brazil during the 1950s and early 1960s. He has to limit himself to the hope—or fear—that in the future the peasants will finally unite in struggle.

Neither editor nor authors, with the exception of Marvin Harris, seems to grasp the dismal impact of their collective effort. Yet this story of failure, ambiguity, or retrogression is the only unity in the book. The juxtaposition of Brazil and Angola implies a historical commonality and it would presumably be found in their Por-

tuguese and African heritage. Yet such a commonality never emerges as an analytical or conceptual device and is specifically denied in papers by Harris and Roger Bastide. Perhaps protest, resistance, or messianism are different in these two areas from such movements elsewhere, but this meaning does not seem to be intended by the authors. Alternatively, the point of comparative study may be not to explore similarities but to detail differences despite them. Yet only Harris devotes attention to such a task, and Ronald Chilcote, the editor, seems to misunderstand his purpose. The conceptual problem is exemplified by Chilcote's attempt to classify the movements of protest and resistance. First he separates Brazilian and Angolan experiences and then he is forced to lump Catholic Action, millenarian movements, banditry, military revolt in the countryside, and communist peasant leagues into the same subcategory. Perhaps the conclusion is that we must abandon the practice, of which I too have been guilty, of publishing multiauthored books that spring from the mind of the editor and not from the collaborative attempt of authors to deal with the same problem. When, in addition to the lack of unity, some of the contributions are disjointed, others badly translated, still others are summaries of findings better presented elsewhere, and the whole is published four years late, the editor may justifiably feel that his energy could have been more profitably directed.

RICHARD GRAHAM
University of Texas,
Austin

RONDO CAMERON, editor. *Banking and Economic Development: Some Lessons of History*. New York: Oxford University Press. 1972. Pp. ix, 267. \$8.95.

There are two theoretically polar positions concerning banking and economic development: (a) a fully developed banking system in an otherwise backward economy and (b) a fully developed modern economy that has no banking system. Examples of either do not exist. Reality lies between. This book examines, with eight case studies, the middle ground between the two extremes using Alexander Gerschenkron's hypothesis as a motivating paradigm. Gerschenkron believes that as economies are (have been) more relatively backward, the greater is (was) the motivating power of banking's entrepreneurial function in engendering those social changes that coalesce(ed) into the fabric of economic progress. In the present volume industrialization is at early stages, too,

in each country examined. The object of these essays is to examine specific economies during periods of economic change and modernization to determine how adequate the Gerschenkron thesis is as a guide to these histories. The authors, economies, and dates are: Richard Rudolph (Austria, 1800–1914), Jon S. Cohen (Italy, 1861–1914), Gabriel Tortilla (Spain, 1829–1914), John R. Lampe (Serbia, 1878–1912), Kozo Yamamura (Japan, 1868–1930), George D. Green (Louisiana, 1804–61), and Richard Sylla (United States, 1863–1913). There is in addition a substantial introduction by the editor, Rondo Cameron, treating the outlines of the theoretical argument, summarizing these empirical results against the theory.

Not surprisingly the Gerschenkron thesis is more useful in some cases than in others. The divergences are surprising and suggestive. What is most surprising though, at least to me, is that all of the authors have managed to weave their narrow concerns into larger questions of economic history in a way that is interesting, even fascinating. All of the essays are sharp, economically written, and supported by quantitative analyses that are the results of hard thinking and scholarly ingenuity. This is a most interesting little book. One might argue that Germany, rather than Austria, or England, rather than Serbia, would have been more useful case studies. I disagree. The question at issue needs to be illuminated by studies of detail, and no amount of theoretical acrobatics at the aggregate level is a substitute. Hence the justification for such studies as these. Testing the Gerschenkron thesis requires detailed analysis at the levels of disaggregation these authors explored. Serbia is as germane a subject as England for this purpose, just as is Louisiana, rather than the whole United States economy.

JONATHAN R. T. HUGHES
Northwestern University

JOSEPH HABERER, *Politics and the Community of Science*. New York: Van Nostrand Reinhold Company. 1969. Pp. vi, 337. \$6.50.

It is the thesis of this book that the politics of the modern community of science were established by the contrasting models of two of its founders, Descartes and Francis Bacon. For Bacon "science is designed to be applied—as a method, a body of knowledge, a social enterprise." For Descartes science is "basic," and the scientist's motive is "to create a universe, as if he were God, and to be able to understand it." Although differently organized as communities, and with different goals, both models of

modern science advocate a relationship to the larger world that Haberer characterizes as "prudential acquiescence." To use his words: "in any serious confrontation with State and Church, that is, with potential enemies who could inflict serious sanctions, they both advocated a tactic of prudential acquiescence: their theory and their conduct posited retreat or an apparent acquiescence as the appropriate response to danger." By way of illustration Haberer recounts the behavior and fate of German science under the Weimar and Nazi regimes, and of American science during the Oppenheimer affair.

Historians of science will be uncomfortable with the fact that Haberer's easy generalizations concerning Descartes and Bacon come from a less than thorough study of the rise of modern science, and political historians will no doubt quarrel with his characterization (or lack of it) of Germany in the 1920s and 1930s, and America in the 1950s. His considerable contribution is not to the traditional history of science, however, but to precisely those important questions to which that discipline seldom addresses itself. His insistence that "politics is an inherent component of the scientific enterprise" clashes directly with the official ideology of modern science—which has been, unfortunately, too often internalized as well by historians of the subject.

It is true, as Haberer points out, that "science still tends to speak with the voice of Descartes and act with the hand of Bacon." It is significant that many working scientists, ignorant of traditional histories of science, have read and worried about Haberer's book. Such charges as "scientists have almost always been pliant partners, willing under almost any condition to accommodate to a given political order," may not arise completely from his two case studies, but they apparently speak to some deep perceptions in the contemporary scientific community. Historians of science would do well to take seriously both Haberer's insights and the response he has found among scientists.

CARROLL PURSELL
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HERMAN H. GOLDSTINE, *The Computer: From Pascal to von Neumann*. [Princeton:] Princeton University Press. 1972. Pp. x, 378. \$12.50.

The computer has become the metaphor and the representative technological device of our age. Dr. Herman H. Goldstine was a participant in the opening stages of the computer revolu-

tion that began only three decades ago. His narrative, although difficult and unnecessarily complicated at times, tells about the invention of the electronic digital computer for the calculation of ballistic tables. But, regrettably, he devotes the first third of the book to pre-World War II computers with the implication that they are progenitors of the modern machine. The computer in use today is no more in a historical line with Charles Babbage's analytical engine than the radio can be called a lineal descendant of the flag semiphor.

Dr. Goldstine's personal account, as he rightly claims, is an essential source for the historian, but that historian will have to be wary of being drawn into the trap of the author's too simple interpretations. His own experience contradicts his belief that new calculating machines came about with the convergence of new technology—gears, relays, or vacuum tubes—and the recognition of the necessity for the advance. The Ballistic Research Laboratory sought a faster means of calculating tables, it is true, but their break-through came when they analyzed the problem into its simplest components, that is, components capable of being handled by electronic circuits doing simple-minded iterative operations. Stating the ballistic tables in the simplest mathematical terms and using electronic circuits was the first stage of the computer revolution.

John von Neumann, with Dr. Goldstine's help, prepared the way for the second stage of modern computer development. By generalizing the computer operation they showed that the key to its success was the two-symbol language. The original specialized computer became the model for all computers that handle large quantities of information. In this second stage the technological break-through became a socially determined innovation employed in an endless number of places, not only in computation of higher mathematics, but in the collection, storage, and retrieval of information. It is in this last function that university students find themselves identified by a number with little personal regard for their idiosyncracies.

Dr. Goldstine and those who worked with him deserve credit for the major technological break-throughs—the first stage. The rest of society bears responsibility for the many uses and abuses of the miracle of modern electronics—the second, socially determined, stage of the computer revolution.

HAROLD ISSADORE SHARLIN
Iowa State University

GEORGE W. CORNER. *Doctor Kane of the Arctic Seas*. Philadelphia: Temple University Press. 1972. Pp. xiii, 306. \$10.00.

Elisha Kent Kane (1820–57), naval surgeon and eldest son of Judge John Kane, a prominent Philadelphia Democrat, was the commander of the second Grinnell expedition of 1853–55 to the Arctic in search of the lost English explorer, Sir John Franklin. With little experience in naval command and only one previous adventure in Arctic exploration, in a series of unwise judgments Elisha Kane subjected his small ship's crew to two terrifying winters of ice-locked semistarvation off northern Greenland. The company suffered scurvy, madness, threat of mutiny, and the secession of a frightened group of the seamen who fled south only to return defeated to their captain. Kane finally led a desperate sledge journey across five hundred miles of ice desert and floe-choked ocean. Befriended by Eskimos, the party reached a whaling village in Baffin Bay, was intercepted by a United States rescue squadron, and was taken to New York for a tumultuous welcome.

The instigator of this expedition was an unlikely hero. Elisha Kane stood five feet four and suffered from recurrent rheumatic fever. He studied medicine after friends and family convinced him that engineering was too rigorous for the life of periodic invalidism he had ahead. He was driven by the contradictory needs of escaping his father's domination and of earning his esteem by a publicly acclaimed deed. The emotional double-bind prolonged his adolescent immaturity, apparently brought neurosis, and fostered the recklessness that would endanger his expedition. Not unexpectedly he was unable to free himself from family opposition to marry Margaret Fox, the famous spiritualist, with whom he had exchanged pledges before his fateful voyage.

George Corner portrays Kane with a minimum of psychological analysis, even though this treatment simplifies a man whom he admires. Rather, Corner emphasizes Kane's scientific pursuits (his medical thesis was published), which were genuine though superficial. The biography is well researched, drawing on family papers, and plainly written, but interest in it will be limited largely to a nonprofessional interest in the rash exploits of its hero.

RONALD C. TOBEY
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W. WARREN WAGAR. *Good Tidings: The Belief in Progress from Darwin to Marcuse*. Bloom-

ington: Indiana University Press. 1972. Pp. viii, 398. \$11.50.

RICHARD KING. *The Party of Eros: Radical Social Thought and the Realm of Freedom*. Chapel Hill: University of North Carolina Press. 1972. Pp. 227. \$7.50.

From the Enlightenment until the First World War the idea of progress pervaded most aspects of Western thought. Since 1914 it has fallen on evil times; its detractors have been numerous and its adherents both fewer in number and more qualified in their enthusiasm. W. Warren Wagar's *Good Tidings* traces the vicissitudes of the belief in progress from the Golden Age of Herbert Spencer to the Freudian radicalism of Marcuse. For Wagar belief in progress is simply a faith, "a thought form" rather than a doctrine with specific ideological content; its salient features are a view of history that traces general improvement in the temporal life of man into the predictable or possible future measured by some standard of good. After wisely establishing his own definition of progress, he gives a brief sketch of its history until *la belle époque* (i.e., 1880-1914), reiterating his previously published contentions as to the modernity of the notion. The book is then divided into three sections: the first deals with belief in progress in *la belle époque*; the second with critics of progress from the nineteenth century until the 1950s; and the third with the survival and subsequent resurrection of the idea from 1914 until 1970. Consequently two separate dialogues are established between the believers and the skeptics on the one hand and between a belief in a process of history and the harsh facts of history itself on the other. The net is cast wide and most aspects of human thought are incorporated—political thought, philosophy, theology, sociology, anthropology, psychology, natural science, literature. Those discussed range from Bergson and Dewey to R. H. S. Crossman and Kurt Vonnegut. Indeed, over one hundred and fifty individuals' views on the notion of progress are discussed. Herein lies the basic problem that confronts the author.

Until the 1880s Western thought had a unity, partially based on the belief in progress; thereafter it has disintegrated under the impact of its own fecundity into numerous "glorious fragments." Had each fragment taken a stand for or against progress, Wagar's task would have been easier. Many major figures' thought, however, is at best ambivalent or almost irrelevant when viewed from the point of view of a belief in progress. Thus Nietzsche appears as both

believer and skeptic; Karl Jaspers, indeed most existentialists, becomes very hard to assign finally to one camp or the other, and often, as in the case of Bonhoeffer, Robinson, and the "secular theologians," optimism and meliorist aspirations are equated to a belief in progress. Figures are often discussed, therefore, because of their stature rather than because they are prophets or critics of progress per se. As a result the book becomes virtually a history of Western thought since 1880, with the idea of progress serving as a compass to guide the way through the morass of its disintegration. As a result some interesting patterns and relationships emerge, and the tendency of the book to become an encyclopedia of modern thought is mitigated.

To cover so many figures is to invite criticism as to inclusions or omissions; but a few individuals this way or that would make no difference. The merely fleeting reference to literary naturalism and total omission of Einstein and relativity theory are slightly more puzzling, although symptomatic of the relative weakness in the treatment of both literature and natural science. Wagar is himself a firm believer in the concept of progress and shows that, understood in terms of an increasing knowledge and control of the natural world, it is still far from a historical curiosity.

One of the many virtues of Wagar's book is that it treats American and European thinkers together and Western thought as a whole. The dilemma facing Richard King in his *The Party of Eros* is maintaining a distinction and balance between European ideas and their American context and transfiguration. King solves his dilemma well. His immediate purpose is to study the way Sigmund Freud's ideas have been used by three radical social thinkers—Paul Goodman, Herbert Marcuse, and Norman O. Brown—in the twenty-five years since 1945. The broader purpose is to reveal the roots of the current counterculture. In an excellent chapter on the Progressives he shows not only how Progressivism helped create the society against which the counterculture is in revolt, but how the Progressives themselves, particularly Dewey and Randolph Bourne by their stress on youth, education, the primitive, and the generational conflict, helped create a vessel wherein Freudian ideas could flow. Given the sterility of American political thought since the thirties and the failure of orthodox Marxism in America, social radicals like Goodman turned to psychology, not economics or politics, to seek the cause of and solution to man's predicament.

King shows, too, that Freudian influence was complex; different people drew on different strands in his thought. It was the early Freud that influenced the "Beat Generation" via Wilhelm Reich and the later Freud that influenced Marcuse. Indeed, Brown, Goodman, and Marcuse are post-Freudians because Freud could not offer any consolation or liberation from repression. King also rather plays down the influence of Marx on Marcuse, tending to see Max Weber as more important. As an exploration of the ideology of the counterculture in the round, it has much to recommend it, but there are problems. Partly these stem from the material itself. Goodman, Marcuse, and Brown have much in common but are also much at odds; it is questionable if they do form a party, of "Eros" or of anything else. Furthermore, King is too defensive. He does not merely criticize the subject matter, he tends to argue with it. Yet this is a useful book not only in showing "where it's at" but, more important, how "it" got there.

PETER J. KING
Carleton University

JAY MONAGHAN. *Chile, Peru, and the California Gold Rush of 1849*. Berkeley and Los Angeles: University of California Press. 1973. Pp. viii, 312. \$11.95.

The book under review is not a scholarly historical work. Rather it is in the genre of popular history of the American West, with the area of coverage simply extended to include Chile and Peru. A companion volume to Mr. Monaghan's earlier *Australians and the Gold Rush: California and Down Under, 1849-1854*, the present work explores the impact of the Gold Rush on Chileans and Peruvians as well as their roles in California. This is an interesting theme, probably meriting serious and systematic treatment. Unfortunately the topic does not receive such treatment here.

Mr. Monaghan has written more with the intent to entertain than to inform. He can never resist a good yarn (or even a not so good one), no matter how irrelevant to his quest. Thus for no clear reason the reader is treated to a four-page sketch of the conquest of Peru, complete with details on the execution of Atahualpa. And his account is sprinkled with equally irrelevant tales taken from nineteenth-century travel accounts.

Even accepting this fascination with the merely picturesque, one is offended by the author's flip tone toward Latin American cul-

ture, most flagrantly and foolishly demonstrated in supposedly literal translations from Spanish that preserve the Spanish linguistic structure for intended comic effect. (" 'Like to me is said, *las malaguas* . . . bait irresistible make for fish. Si, Senor?' ") As it turns out Mr. Monaghan's own grasp of Spanish is as "funny" as the structure of the Spanish language. ("Plaza des Armes," "Plaza des Gallos," etc.)

As to the basic content the narrative is most interesting in its treatment of matters in California. Mr. Monaghan thinks that anti-Chilean riots in the Gold Rush occurred because the Chileans got to some of the best claims first and were more effective and experienced miners than the other groups. Unfortunately his evidence for Chilean experience in gold panning is weak and specious. On Chile and Peru, Mr. Monaghan's treatment, based primarily upon standard Chilean works, travellers' accounts, and a few contemporary newspapers, is much less competent. He discusses but explains poorly why Chileans participated more actively in the Gold Rush than Peruvians. He is disappointed that the Gold Rush experience had little political impact upon Chile and Peru and only sloppy analysis enables him to suggest any at all.

I suppose that the University of California Press has issued this book in the hope that it will make money to pay for more serious works. Even if one accepts this motive the press ought to have its editorial wrist slapped for publishing a work that, in intellectual terms, is stunningly sloppy.

FRANK SAFFORD
Northwestern University

JACK M. SCHICK. *The Berlin Crisis, 1958-1962*. Philadelphia: University of Pennsylvania Press. 1971. Pp. xix, 266. \$9.50.

The theme of Jack M. Schick's book is Soviet-American diplomacy. Berlin is his vehicle for displaying it. For a generation the city was a barometer registering fluctuations in the Soviet-American relationship. Whether Berlin remains the place where "the United States is more dependent on nuclear retaliation to ensure a national commitment" is another matter. But even if the commitment is no longer clear this study dramatizes the importance of that divided city at a time when it was the sorest issue between the superpowers.

The anomaly of West Berlin isolated from the Federal Republic by over one hundred miles of East German territory yielded a series

of crises between 1958 and 1962. In reviewing them Schick has brought into focus Dulles's insensitivity toward Russian fears of nuclear weapons in Germany, Khrushchev's reckless threats of nuclear destruction, Eisenhower's embarrassment over the U-2 affair, the meaning of the Berlin Wall, and finally, the intimate connection between Berlin and the Cuban missile crisis of 1962. His conceptualization is impressive, and he presents his analyses clearly. Occasionally his eye-catching subtitles go awry. For some reason he casts the Dulles-Khrushchev contest in the language of cricket, which seems an inappropriate sport to associate with these particular Russian and American statesmen.

The author's detachment is attractive. He is neither a cold warrior nor a New Leftist, although his approach may give more comfort to the former than to the latter. Although he ascribes Soviet aggressiveness to understandable concerns about German membership in NATO he also points out Khrushchev's succumbing to the dangerous temptation to exploit the West at its most vulnerable point on the mistaken assumption that the missile gap favored the Soviet Union. The record itself is one of repeated failure of statecraft on both sides in which fear of a nuclear holocaust stayed war, but not before they grappled at the brink. The worst cases of brinkmanship occurred under Kennedy over the Wall and Cuban missiles. Schick concludes that the Wall was no overnight coup, but a gradual development that accelerated when the United States failed to respond, and that the Soviet-Cuban adventure was intended to resolve the Berlin question. Soviet rashness was matched by American lack of clarity over objectives, although he applauds Kennedy's firmness and restraint over Cuba.

Sources, or lack of them, provide some difficulties for the author. He is forced at times into conjectures about Khrushchev's behavior that may not be correct. The European view of the conflict is relatively neglected. The German reaction is undeveloped, and German source materials are meager. Nevertheless, this is a contribution historians should turn to in untangling the interlocking crises of this period.

LAWRENCE S. KAPLAN
Kent State University

ROBERT M. SLUSSER. *The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June-November 1961*. Baltimore: Johns Hopkins University Press. 1973. Pp. xvi, 509. Cloth \$17.50, paper \$8.50.

ANATOL RAPOPORT. *The Big Two: Soviet-American Perceptions of Foreign Policy*. (American Involvement in the World, volume 1.) New York: Pegasus. 1971. Pp. 249. \$6.95.

The authors of these two books attempt to bring new perspectives to American-Russian relations during the cold war. Anatol Rapoport, a mathematical biologist, ventures boldly into historians' terrain with an incisive interpretation of the confrontation between the United States and the Soviet Union in the past generation. His interpretation of the cold war is unmistakably revisionist. He emphasizes the Soviet Union's weak and essentially defensive posture after the Second World War, Truman's reversal of Roosevelt's cautious hopes for post-war Soviet-American cooperation, the Americans' half-hearted efforts to share atomic secrets and control atomic weapons with the Russians, and their deep-seated desire to impose their nation's liberal ideals and institutions on foreign peoples. He also calls the American political establishment "arrogantly antidemocratic" and criticizes traditional and "realist" scholars and American policymakers alike for their celebration of military strength and power politics.

Although Rapoport stresses America's offensive and aggressive foreign policies, he is less interested in assessing blame than in trying to persuade Americans to adopt new perspectives on international relations. Like many left-liberal and radical scholars, he focuses on the economic and social "substructure" or "roots" of American foreign policy, though he gives this approach the name "systemic." "The prime mover" of American diplomatic history, he asserts, "has been the need to expand . . . ; the so-called 'national interest' of the United States has been shaped to preserving and extending global capitalism." The Russians, on the other hand, had "no practical program for bringing the Communist world order into being." "In short," Rapoport concludes, "while the American program of economic penetration was completely in accord with American political ideology (liberalism), there was no concrete program that could be geared to the supposed long-term goals of Communist ideology. . . . The Soviets lack the powerful instruments of economic penetration that are at the disposal of the United States."

He also uses social psychology to link the dynamic aspects of expansive capitalism to the national character. "People have an urge to engage in exciting collective activity," he suggests, and because "the extreme individualistic ideology" in the United States has progressively

eroded community life and a sense of national purpose, Americans invented an anticommunist menace after 1945 to continue against the newly-proclaimed adversary USSR the sense of national purpose that had begun to fade with the defeat of the Axis powers. Moreover, the American civilian elite tolerates "the self-propelled burgeoning of the American war machine" because it appeals to their capitalistic faith in business and growth.

Despite his numerous provocative insights Rapoport is not entirely persuasive. He has used almost entirely secondary sources, and even these he often introduces as whipping boys for his own criticisms. Rapoport is most convincing in the negative stance of devil's advocate, but his own unconventional hypotheses lack the breadth of research and sustained theoretical musings of leading revisionists like Gabriel Kolko and Richard J. Barnet. Unlike many critics of American foreign policy, Rapoport attempts to evaluate the dynamics of Soviet foreign policy as well, but he is not very successful. His characterization of Nikita Khrushchev as a good-hearted man genuinely interested in détente with the United States is intriguing and plausible but is too thinly researched and speculative to be taken seriously.

Although Slusser, a historian of Russia, shares Rapoport's interest in recent Soviet diplomacy, in other respects his approach to cold-war tensions is markedly different. Rapoport covers twenty-five years of Soviet-American relations in 219 pages of text, while Slusser requires more than twice as much space to write a conventional narrative history of a six-month period during the Berlin crisis of 1961. True, Slusser's detailed analysis of Soviet policy is revisionist. He argues convincingly that deep fissures in the Soviet leadership between Premier Khrushchev's moderate faction and a hard-line group led by Frol R. Kozlov surfaced during the Berlin crisis and seriously threatened its peaceful resolution with the West. His detailed description of the volatile political situation adds an important dimension to recent Soviet-American relations.

But Slusser's long quotations, needless repetition, and labored prose, however, often obscure the significance of the events for an understanding either of Russian or cold-war history. Presumably the author believed his controversial thesis required exhaustive discussion of the entire story so that he could effectively parry all possible objections of Russian specialists. This preoccupation with minutiae may be defensible in elaborating the complexities of

Soviet politics, and the last portion of the book that meticulously chronicles the climax of the internal power struggle at the Twenty-second Party Congress is a valuable contribution to recent Soviet history. But the Soviet-American confrontation over Berlin is of minor significance by comparison, especially because the positions of the two powers had essentially stabilized very early in the crisis. Although Slusser's volume is a model of intensive research, intelligent speculation, and patient exposition, the tedious recapitulation of the moves and countermoves of the great powers is likely to disappoint many readers before they get to his analysis of the internal party strife in the Kremlin.

DAVID S. PATTERSON
Rice University

International Bibliography of Historical Sciences. Volumes 37-38, 1968-1969, including some publications of previous years. Edited with the contributions of the national committees by MICHEL FRANÇOIS and NICOLAS TOLU for the International Committee of Historical Sciences, Lausanne. (Published with the assistance of UNESCO and under the patronage of the International Council for Philosophy and Humanistic Studies.) Paris: Librairie Armand Colin. 1971. Pp. xxviii, 654.

These two-volumes-in-one of the *International Bibliography of Historical Sciences* represent an effort by the International Committee of Historical Sciences to bring the bibliography up to date. The last previous volume (vol. 36, 1967) was published in 1970, three years after the date of publication of the items entered in the bibliography. Volumes 37-38 (1968-69) appeared only two years after the publication of the entries for 1969. The same plan will be followed for volumes 39-40 (1970-71). Thereafter the pattern of one volume per year is expected to be resumed.

Unfortunately the quickened publication rate has resulted in a reduction in coverage. Prior to the appearance of this double volume, the number of entries in the annual bibliography had grown steadily—from 4,908 in volume 1 to 8,390 in volume 36. But volumes 37-38 together have only 10,945 entries, and a comparable limitation will apparently be placed on volumes 39-40.

Dismal predictions for the bibliography's future emanate from responsible officials. Funds are increasingly inadequate. Julien Cain, president of the Bibliographical Commission of the International Committee on Historical Sciences,

which supervises preparation of the bibliography, pleads for help from the 14th International Congress of Historical Sciences that will meet in San Francisco in 1975.

The American Historical Association, which represents the historians of the United States on the International Committee, sponsor of the Congress, should initiate action to increase the support provided for the *International Bibliography*. The AHA's current annual contribution to the International Committee is 500 Swiss francs, approximately \$150. Surely the foremost historical organization in the world's richest nation can afford to contribute a significantly larger amount than that.

Increased support now by the AHA for the *International Bibliography* would help to offset several decades of indifference toward that publication. It would also be in harmony with the renewed interest at the AHA in historical bibliography. Although the organization's commitment to a new annual listing of periodical articles on American history probably precludes its soon undertaking large-scale projects for history in general, the AHA could reassert the leadership it once held in the field of international historical bibliography and by example and persuasion make certain that the 14th Congress takes steps not only to assure the survival of the *International Bibliography* but also to foster its growth.

Because of changes taking place in the researcher's environment in the United States, historical bibliographies are likely to be of greater service in the future than in the past. As college campuses grow, history professors cannot rely on having offices and classrooms a few strides away from the library. Those who once could walk quickly to the library to use its large reference sets and its card catalog, long the historian's subject bibliography, have already in some instances been moved blocks away. Students have known this handicap for years, and many of them are being steadily housed at still greater distances. Similarly, the large, well-equipped public libraries are becoming more difficult to reach in the burgeoning cities. Even more serious is the questionable future of card catalogs. In some libraries, personnel who seldom use the tools of research have been placed in charge of making decisions about catalogs and are attracted to space-saving book catalogs, which are cumbersome and frustrating to the regular researcher. If the card catalogs cannot be saved, historians will be especially grateful to have bibliographies on their own shelves.

A fully developed *International Bibliography of Historical Sciences* could be the pinnacle of a pyramid of historical bibliography. At the base, in the United States, would be the book reviews, book lists, and article lists in historical journals; the new annual list of articles on American history undertaken by the AHA; and the abstracts of articles in ABC-Clio's *Historical Abstracts* and *America: History and Life*. In the middle could be a revived and expanded *Writings on American History*, for example, or the "American History Index" proposed by ABC-Clio; but either alternative could omit articles and restrict its coverage to such items as books, dissertations, proceedings, and pamphlets. At the top could be the *International Bibliography*, a compilation—possibly selective rather than all-inclusive—of items already identified at the other levels. If all the nations participating in the International Committee built such a bibliographical structure and thus enriched their contributions to the *International Bibliography*, it would be a special treasure for historians and libraries all over the world.

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ANCIENT

DEREK ROE. *Prehistory: An Introduction*. Berkeley and Los Angeles: University of California Press. 1970. Pp. 288. \$6.50.

GRAHAME CLARK. *Aspects of Prehistory*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xiii, 161. \$5.95.

Prehistory is today a popular subject. As a result sometimes a book on prehistoric archeology is published whose *raison d'être* is unclear. *Prehistory: An Introduction* is perhaps an example of this.

I am not sure to whom this book is addressed; the title suggests one audience, the content another. Certainly it is not a general introduction to prehistory or to what prehistorians are doing and thinking. The author admits this in his preface, but not loudly enough. The dust jacket almost says the opposite. Actually the book is a traditional tour guide to the high spots of British prehistory with barely enough on Europe and other parts of the Old World thrown in to set the British story in perspective. The chapters are arranged chronologically, with the many "cultures" marching along like soldiers on parade. More dynamic issues such as the na-

ture of culture and the processes and causes of cultural change are not discussed, and the reader is left thinking that the prehistorian is still little more than a cataloger of ancient pots and pans. All this makes some sense if one is writing an introductory grammar of British prehistory for the beginner, yet the University of California Press was certainly hoping to catch a larger audience. On the other hand, readable introductions to British prehistory are already available that are perhaps better because they are more sharply focused. One wonders if the author was led astray by his publisher.

These strictures aside, the book is well written, the illustrations are adequate, the bibliography is excellent, and the notes add much to the value of the whole. Indeed in the notes the author gives us a glimpse of the book he could have and perhaps should have written. Had the whole been pitched to the level of the notes, and had it dealt in some detail with many of the issues only touched upon in those notes, then the author would have caught a wider audience and we would have a better and more useful book.

Aspects of Prehistory is a slightly expanded and documented version of three lectures given by Professor Clark at Berkeley in 1969. Here it is clear what audience is addressed; both lectures and book are for the interested, educated general public. Though the book contains little the practicing prehistorian will find new, some professionals will benefit from the straightforward and organized way in which Clark puts his case.

The author is concerned with certain broad, general concepts that may be said to emerge from the study of prehistory. The first chapter, "The Relevance of Prehistory," explores the proposition that "world prehistory makes it sufficiently plain that both literacy and urban living are extremely recent. From the perspective allowed by prehistory we are all on much the same level" (p. 50). The second chapter outlines man's material progress since becoming a tool user some two million years ago. It is here that Clark most clearly articulates his underlying assumption, which is that our social and material development from the Lower Paleolithic to the present is simply the working out, in a different matrix, of processes of change similar to those of biological evolution. The outstanding aspect of this pattern of social and technological evolution is a progressive growth of man's self-awareness. The final chapter, the

best in the book, is a history of that growing self-awareness and a discussion of its implications for the past, present, and future of man. To become self-aware is to become human, and "societies of human character have been better fitted for survival in a cultural milieu precisely to the extent that they are human; and the record of archaeology testifies over the last few tens of thousands of generations to an increasingly human pattern of behavior" (p. 146). Though I do not entirely agree with the argument, as a prehistorian I appreciate the point and would suggest that these ideas are worth thinking about.

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KATHLEEN KENYON. *Royal Cities of the Old Testament*. New York: Schocken Books. 1971. Pp. xi, 164. \$10.00.

The title of this book is taken from Dr. Kathleen Kenyon's lecture delivered in 1965 before the Palestine Exploration Fund in connection with its centenary celebrations.

The volume, which is primarily archeological, is up to high professional standards. Numerous photographs enable the reader to understand the material concretely. Maps and charts help him locate the places, visualize the terrain, and view ground plans and elevations.

Constant attention is paid to historic setting based on primary sources, which are mainly but not entirely Biblical. The chronological statement on page x and the table on page xi will aid the reader in sorting out the details and arranging them in sequence.

Technical terms are often defined for general readers. For example, though every student of Old Testament problems knows what "high-places" are, Miss Kenyon takes the trouble to define them as the generic designation of heathen shrines (p. 117).

While we could include more sites among "the royal cities of the Old Testament" than the author does, no one should quarrel with her because she states exactly what she means and gives her reasons. Thus 1 Kings 9:15 groups Jerusalem, Hazor, Megiddo, and Gezer as special royal cities of Solomon. The only addition the author makes is Samaria, the main capital of the northern Kingdom of Israel.

The book includes the author's presentation of her own excavations in Jerusalem before the 1967 war. Her account of archeological work in Jerusalem provides the background for the

later monumental expeditions of Benjamin Mazar, which are not mentioned. The bibliography records no item published after 1966.

R. A. S. Macalister's publication of his diggings at Gezer was long revered as "the Bible of Palestinian archeology" and still retains considerable importance. Yet his work at Gezer was so lacking in stratigraphic evidence that Miss Kenyon feels obliged virtually to omit a discussion of that site. The book before us therefore deals only with Jerusalem, Megiddo, Hazor, and Samaria.

Miss Kenyon formulates her methods scientifically and clearly (for example, her careful "assessment of pottery evidence" on p. 122). Yet with complete integrity she points out again and again that even the most methodical archeological work often leads only to very tentative conclusions.

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CARL ANDRESEN. *Einführung in die christliche Archäologie*. (Die Kirche in ihrer Geschichte, volume 1, series B, part 1.) Göttingen: Vandenhoeck & Ruprecht. 1971. Pp. 175. DM 32.50.

Few scholars could match the broad erudition of the distinguished Church historian, Carl Andresen, and few would be better qualified to write a handbook on the art and archeology of the Church. The present work is not an introduction to Christian archeology for laymen. The *Einführung* is instead a valuable reference work for scholars who wish to begin the investigation of a particular aspect of Christian art and archeology.

Andresen's monograph is divided into two major parts. The first (pp. 11-62) consists of detailed bibliographies with brief discussions of painting, sculpture, architecture, etc. The second section (pp. 62-142) is a survey of special themes from the pre-Constantinian period to the Age of Justinian.

The scope of the book is staggering and the number of works cited is stupefying. The author has attempted to survey fields of study covering areas from England to Iraq and periods from the first to the ninth century A.D. Scholars will be indebted to Andresen for sharing with them the accumulated notes of a lifetime of scholarship.

In view of the inherently impossible task of mastering all fields and the exciting but frustrating accretion of data provided by current excavations, there are some striking deficiencies

in Andresen's work. Some of these lacunae may be understood in view of the limitation of space. There is, for example, a listing of works on the excavations at St. Peter's in Rome (pp. 68-69), but no discussion of this significant and controversial subject.

Some deficiencies, however, seem related to the author's personal interests and competence. He is most instructive in dealing with the later periods of Church history but he is not a helpful guide for the archeological developments in Palestine at the dawn of Christianity. In a discussion of the pre-Constantinian period (p. 63) his earliest archeological citation is a catacomb at Rome from the second century A.D.!

Though Andresen acknowledges the importance of Jewish prototypes for Christian art and archeology, his references to Jewish monuments leave much to be desired. His bibliographical list on Jewish mosaics (p. 14) contains no reference to important mosaics excavated by Yadin at Masada from 1963 to 1965. In the bibliography on synagogues (p. 30) it is surprising to find no references to the important first-century synagogue at Masada or to the enormous synagogue discovered at Sardis.

Important archeological evidences for early Christianity in Palestine are also neglected. Though the recent excavations at the Church of the Holy Sepulchre are noted, no notice is taken of Kenyon's excavations at the nearby Muristan that help to confirm the authenticity of the site as the scene of Christ's crucifixion and burial. References to ossuaries and their symbols, which may possibly provide evidence for early Christianity, are lacking. Nor is note taken of such important discoveries in Israel as the first epigraphical documentation of Pilate found in 1961 and of Felix the procurator found in 1966. As Andresen completed his manuscript in the fall of 1970 he may not have known of the first archeological evidence for crucifixion, discovered in Jerusalem in 1968 and published in the *Israel Exploration Journal* in 1970.

It seems that the surprising omission of the latter journal in the list of recommended periodicals (p. 34) and the listing of the *Bulletin of the American Schools of Oriental Research* inexplicably only up to the 1957 issue indicate a blind spot in the author's almost all-encompassing vision. This is most unfortunate inasmuch as some of the most significant archeological developments for the early history of Christianity, such as the excavation at Pella and the discovery of Byzantine churches near Amman,

are being made in the areas of Israel and Jordan—the birthplace of Christianity.

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M. I. FINLEY. *Early Greece: The Bronze and Archaic Ages*. (Ancient Culture and Society.) New York: W. W. Norton and Company. 1970. Pp. 155. \$5.00.

Moses Finley, the professor of ancient history at Cambridge University, sketches here the history of Greece from earliest times to ca. 500 B.C. in a book of 150 pages. Such a scope does not make for an expansive treatment, but he has made good use of his space and it is clear that Finley, the general editor of the series to which this book belongs, has written the book he wanted to write. This work is not the result of boiling down a long manuscript to manageable size, nor is it a potted history of Greece.

What Finley does is to give the reader his own interpretation of the historical development of Greece prior to the Persian Wars, consciously setting the stage for what is to follow. He concentrates upon what he considers to be the essential characteristics of each period and makes no attempt at narrative history. In sixty-eight pages Finley summarizes the history of Greece before 1200 B.C. emphasizing the palaces of Minoan Crete, the shaft graves of Mycenae, and the writing systems of the Bronze Age Aegean. His account is balanced and up to date, including a discussion of the new-cult objects from Mycenae (p. 57).

Finley is very skeptical about mass migrations and total displacement of populations, stressing the importance of commerce in the foreign expansion of Greece. The Greeks were forced to go abroad for many of their basic needs, especially metals. Finley's explanation of the Minotaur legend (p. 40f.) is not very convincing, and I certainly would not agree with the statement (p. 45) that "Late Minoan II saw Knossos at the height of its power" (and why Knossos, when Knossos has become standard and is so preferable?). Any discussion of the Greek Neolithic must now include the material from the Franchthi Cave in the Argolid (compare, T. W. Jacobsen, *Hesperia*, 42 [1973]: 45-88). The most significant recent publication dealing with the Bronze Age Aegean is Colin Renfrew's *The Emergence of Civilisation. The Cyclades and the Aegean in the Third Millennium B.C.* (London, 1972).

As in a number of other publications, Finley

is very critical of those who are trying to turn Greek legends and myths into history. Finley himself has recently been taken to task by one of his favorite targets, George Mylonas—in Mylonas's *Mycenae's Last Century of Greatness* (Sydney, 1968). I am in full agreement with Finley's position and endorse his remark (p. 10) that: "One is free to believe if one wishes that King Minos of Knossos, Agamemnon of Mycenae and Priam of Troy were historical personages, not figures of myth; no one has found them on the spot in any shape whatsoever, not even as a name on a slab or a seal-stone." As for the "Age of Homer," attention should be given to the discussion by A. M. Snodgrass (*Gnomon*, 42 [1970]: 157-66).

The "Dark Age" of Greek history has been treated in recent books by A. M. Snodgrass and V. R. d'A. Desborough, both published in 1972. The fall of Mycenaean civilization, the Trojan War, the Dorian Invasion, and the transition from Bronze Age to Iron Age all still present the historian with many problems. Finley follows recent interpretations, especially that of C. G. Starr, in emphasizing the formative nature of the obscure centuries between 1200 and 800 B.C. Protogeometric pottery, the introduction of cremation in burials, and the spread of the use of iron are the age. The significance of possible contacts with Europe is still an unresolved question. Desborough emphasizes the presence of northern invaders, while Snodgrass denies their very existence. Finley is ambiguous: northern invaders would provide a welcome explanation, if only their existence could be substantiated.

The Archaic period (800-500 B.C.) is dominated by the introduction of hoplite warfare and the rise of the tyrant. Hoplites fighting in phalanx formation meant an increase in the number of men bearing arms; fighting for the polis led to demands for a voice in the affairs of that polis. Finley stresses the popularity of the Greek tyrants and the paradox that, by breaking the habit of aristocratic rule, the tyrants actually helped to bring about democratic government (p. 107). The fairly abrupt transitions from monarchy to oligarchy to tyranny to democracy are all related to the social upheavals resulting from overpopulation amid new social and economic pressures. The most characteristic response was colonization, a movement that cannot be explained in terms of trade and the search for new sources of metal (p. 97).

Early Greece concludes with separate chapters on Sparta and Athens. While this emphasis

provides a convenient introduction to the Classical period, when Greek history was dominated by those two states, it does give a misleading conception of the Archaic period itself, a time when states such as Argos and Corinth were as important, if not more important, than either Athens or Sparta. The interested reader would do well to consult W. G. Forrest's *Emergence of Greek Democracy, 800-400 B.C.* (1966), an excellent account not even listed in Finley's brief bibliography.

In these days of lavish books, thin on text but rich in gorgeous color illustrations, it is a pleasure to find a book that has the text as its central concern. *Early Greece* is not a handsome book, not a "tribute to the skill of the modern art of printing," but the printing is adequate and the book provides an excellent introduction to the history of pre-Classical Greece at a very modest price. It only remains to add that the book is now available in a French translation, as *Les premiers temps de la Grèce, l'âge de bronze et l'époque archaïque* (Paris [François Maspero], 1973). This includes a few minor corrections and additions to the text as well as a revised bibliography.

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CHIARA PECORELLA LONGO. *"Eterie" e gruppi politici nell'Atene del IV sec. a. C.* (Università di Padova, Pubblicazioni della Facoltà di Lettere e Filosofia, volume 48.) Florence: Leo S. Olschki Editore. 1971. Pp. 162. L. 3,500.

In 1913 George Miller Calhoun had his doctoral dissertation, *Athenian Clubs in Politics and Litigation*, reprinted from the University of Texas Bulletin, number 262, and that monograph remains today a starting point for investigations of this elusive phenomenon at Athens. The earlier clubs were societies made up of men whose backgrounds, interests, and age were about the same. Sometimes the clubs took the form of sworn associations and were so named. In Athens during the sixth and fifth centuries B.C. they were important units of social and political action. From Thucydides especially, we know the part they played in the oligarchic revolution of 411 B.C. Then in the fourth century B.C., although their presence can be detected at publicly and privately motivated legal trials, they seem to have lost the sort of coherence and power that changes governments.

Now Chiara Pecorella Longo in her own doctoral dissertation reviews the comparatively abundant evidence for political life in fourth-

century B.C. Athens to test the traditional view that the clubs (*hetaireiai*) were active and influential only in restricted spheres after ca. 403 B.C. She accordingly surveys possible references to such associations in Plato, Isocrates, Xenophon, and Aristotle (ch. 1); argues that the impeachment law preserved in an oration of Hyperides was re-edited in 403-02 B.C. after its initial formulation in 411-10 B.C. (ch. 2); discusses the political motivation of Andocides's trial in 399 B.C. (ch. 3); enumerates various groups associated with various Athenian political leaders (chs. 4-7); and concludes that the traditional view is correct. Clubs lost much political power in the fourth century and were most active in law courts. Dr. Longo adds that certain words or phrases that in earlier times were used to denote clubs, in the fourth century sometimes referred instead merely to groups formed for financial or political reasons and identified with a particular man.

Dr. Longo's acquaintance with the literature, both ancient and modern, is wide ranging if not exhaustive. She could, for instance, have speculated on the contents of the *defixionum tabellae* from Athens that are relevant to her theme. Her assessments of modern scholarship are, however, generally sensible. Her conclusions have the sanction of long usage and reputable approval, and her study can be cited as a responsible confirmation of a useful hypothesis.

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K. D. WHITE. *Roman Farming*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1970. Pp. 536. \$12.50.

In the Roman imperial period, as throughout the history of the ancient Mediterranean world, agriculture formed the economic basis of society. The vast administrative structure of the Empire relied upon an agricultural surplus, and agriculture was of similar importance in the private sector of the economy. As under the Republic, although farm management was *inliberalis labor*, land was the primary form of investment. By the *Lex Claudia* of 218 B.C. senators were forbidden to own ships other than small ones to transport the produce of their estates; and throughout its existence the senatorial class remained a landed aristocracy. The pervasive importance of agriculture in the economic life of ancient Italy secures for it a historical significance far above other aspects of ancient industry and technology. Agrarian

reform is a leitmotiv in the social struggles of the early Republic and in the century following the Gracchi, while the extent and causes of agricultural decline in late antiquity is essential to any study of the decline of Roman power in the West.

In this context ancient political historians, as well as social and economic historians, will find a valuable tool in K. D. White's encyclopedic survey of agricultural techniques and organization in Roman Italy. After a brief critical exposition of the literary and archeological sources and a summary of the natural resources of the various regions of Italy, White proceeds to a detailed treatment of the variety of soils available for cultivation and methods used in antiquity to maintain soil fertility, including crop rotation, fertilizers and manures, and land drainage and irrigation. A major portion of the text is devoted to the theory and practice of field crop husbandry, arboriculture, horticulture, and animal husbandry. The work concludes with discussions of agricultural personnel and management, systems of farming, size of farm units, agricultural architecture, and the level of technical achievement obtained in Roman agriculture. The absence of a bibliography is regrettable, for all may not have access to White's full and critical *Bibliography of Roman Agriculture* (University of Reading, 1970). In the same way the reader must supplement White's disappointingly brief remarks on farm equipment by reference to the same author's *Agricultural Implements of the Roman World* (Cambridge, 1968). The text is complemented by a series of appendixes, treating such topics as methods of protecting stored grain, comparative estimates of labor productivity, and soil conditions appropriate to particular crops.

White's *Roman Farming* is a masterly survey of a complex topic, and it is of enormous value as a reference work for the present. Nonetheless its treatment of many problems, some of considerable historical interest, must be regarded not as exhaustive and authoritative expositions but rather as preliminary sketches and guides to future research. Although generally judicious in his handling of the sources, White is perhaps overly enthusiastic in his acceptance of the precepts of Roman agricultural writers as evidence for contemporary agricultural practice (compare, Columella 1. praef. 4-5, 7, 13). At points White is not sufficiently critical in his use of nontechnical sources, for example, accepting without reservation (p. 320) the testimony of the *Historia Augusta* concerning Aurelian's free distribution of pork. White

underestimates the enormous cost of land transportation and the role it played in marketing (compare, A. H. Jones, *The Later Roman Empire, Two Eighty-Four to Six Hundred Two* [Cambridge, 1964], pp. 841-55). Surprisingly White gives almost no attention to deforestation, and there is no discussion of exciting new material derived from the analysis of sedimentary deposits (compare, J. B. Ward-Perkins, *Landscape and History in Central Italy* [1964], pp. 2-11). The evidence for free contract labor is far more extensive than White will allow (p. 368). The explicit remarks of Varro (*De re rustica* 1.17.2) and others (Pliny 14.10; Suetonius *Vespasian* 1.14 etc.) can be supplemented by epigraphical testimony (*CIL* VIII.11824).

These strictures, however, should not detract from the fundamental importance of White's work of synthesis and the value of this study as a major contribution to ancient economic history and more particularly to the study of ancient technology.

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R. M. OGILVIE. *The Romans and Their Gods in the Age of Augustus*. (Ancient Culture and Society.) New York: W. W. Norton and Company, 1969. Pp. 135. \$5.00.

In the conclusion of his useful monograph Ogilvie observes that "the smoke no longer curls up from the sacrifices in the Forum; the augur no longer takes his seat on the Capitol to watch the birds wheeling overhead. Yet its ultimate failure should not tempt us to underestimate the validity of Roman religion." Certainly no one will be so tempted after reading this book.

Although the title of the work is *The Romans and Their Gods in the Age of Augustus* it should be quickly pointed out that the book is broader in scope than its title indicates. While the author's express goal is to illustrate how Roman religion could claim the faith of the Augustan age, it is necessary in dealing with a subject so complex as Roman religion to range far afield in time and topic. In eight chapters, which cover the gods, prayer, sacrifice, divination, the religious year, private religion, the priests, and religion in Augustus's time, Ogilvie moves easily across the whole area of the Roman religious experience to provide a literate survey that, though clearly designed for the layman, may be read with profit by the more casual reader and specialist alike.

Thus, for example, Ogilvie ranges from the elder Pliny's remark that "trees were the temples of the spirits and . . . simple farming communities even now dedicate an outstanding tree to a god." to the observation that "the greater gods had their favorite haunts many of which were overseas as a result of the identification of Greek and Roman gods and the resulting attachment of Greek myths to the old, native beliefs."

That Augustus undertook, as part of his general program to heal the wounds of a hundred years of civil war, a broad range of activities designed to revitalize Roman religion is well known. It is Ogilvie's belief, which he carefully documents in the chapter that he devotes specifically to the Augustan period, that the Augustan religious revival was not simply something arbitrarily imposed on the Roman people, but, rather, something that had deep emotional appeal to a tired and confused people who had lost much of their self-confidence. Surely he is right in taking this view, and surely he has performed a good service in providing us with this unpretentious reappraisal of an area a knowledge of which is essential to the understanding of the recovery of confidence by the Romans under Augustus.

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O. A. W. DILKE. *The Roman Land Surveyors: An Introduction to the Agrimensores*. New York: Barnes and Noble, 1971. Pp. 260. \$12.50.

Land surveying was very important for the Romans, with their strong practicality and sense of property. Dilke's appendix A lists thirty-five works in the corpus of ancient treatises on the subject. Their authors, the *agrimensores*, run from Frontinus in the late first to Boethius and Cassiodorus in the first half of the fifth century A.D. The survival of such technical and specialized pieces when so much of Latin literature is lost attests to the concern for land surveying on the part of both the Romans and the medieval scholars who decided what to copy from the materials that they inherited from the late Empire. Modern historians of Rome may, however, find the works of the *agrimensores* of little significance except insofar as centuriation, the laying out of allotments, was a regular feature of the founding of colonies. To this topic Dilke devotes three chapters, 6, 10, and 14.

For those who are interested in Roman surveying, Dilke begins with a study of the back-

ground in Near Eastern and Hellenistic procedures. He then describes the training of Roman surveyors, the instruments that they used, the actual measurement of land and the establishment of boundaries, mapping, and finally the archeological and epigraphical evidence. The archeological evidence consists chiefly of traces of boundaries (*limites*) of centuriation plots as revealed either on the ground or from the air. The epigraphical evidence comprises actual boundary stones and several inscribed surveys (*cadasters*) from Orange in France. It should be noted that the technical chapters are not easy for a layman to follow, particularly that on the instruments used (ch. 5). The general reader will find more comprehensible and informative the chapters on land measurement and centuriation in relation to colonies and state domains.

The book is fully illustrated. It offers reproductions of miniatures and drawings from the medieval manuscripts of the *agrimensores*, illustrations whose originals appear to date from the late Empire. There are also photographs and drawings of surviving instruments, of traces of centuriation as viewed from the air, and of relevant stones and inscriptions. At the end of the book is a list of the locations of surviving sources. On this it should be observed that the fragments of the *Forma Urbis Romae*, the well-known Severan plan of Rome, are not on the wall of a garden of the Museo dei Conservatori; what is there is a copy of Lanciani's reconstruction (1893-1901). The original fragments of the *Forma* were studied by G. Carettoni and others for a new publication in 1960 and are still in the Palazzo Braschi (the Museo di Roma). The book concludes with a glossary, notes and references, two appendixes (on the contents of the corpus, and on the meaning of *kardo* and *decumanus*), a bibliography by chapters, acknowledgments, and an index.

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DAVID P. JORDAN. *Gibbon and His Roman Empire*. Urbana: University of Illinois Press, 1971. Pp. xv, 245. \$8.95.

In the seven essays that make up this book, Jordan discusses Gibbon's life, his use of contemporary scholarship, his relation to the writers and ideas of the Enlightenment, the influence upon him of five earlier writers (Tillemont, Pascal, Bayle, Tacitus, and Montesquieu), and Gibbon's own treatment of Constantine. A brief summary chapter surveys Gibbon's explanations

of the fall of Rome, and discusses the extent of Gibbon's identification with his subject.

It should be said at once that this is an interesting and useful book: it is well written and organized (despite a tendency to repetition); accurate (checking has revealed no mistaken references, and, other than words in Latin, there are very few misprints); and Jordan is both judicious in his use of sources and careful in his acknowledgment of debts.

The book, nevertheless, serves rather as an introduction to several aspects of Gibbon's life and work than as a major contribution to our understanding of Gibbon. A number of the chapters are based, at least in part, on previous studies, as Jordan himself generously acknowledges. Thus, the study of contemporary scholarship owes much to Momigliano; Gibbon's gradual development of an ironic narrative mode has been noted by Braudy; and Jordan himself has previously dealt with Gibbon's treatment of Constantine. Again, most of the essays are left as rather general treatments, and Jordan is not overly concerned with either detail or documentation. Thus, Pascal's Jansenism and Gibbon's reaction to it are described in five pages without a single reference to Pascal's works and only one to Gibbon's (pp. 149-54); intellectual influences on Gibbon are discussed here in a manner that is neither so detailed nor so incisive as Dawson's earlier work; occasionally topics are not specifically discussed at all, as with the sources of Gibbon's picture of Constantine (surely, for example, Constantine's hypocrisy in hiding his true character as a young man is much influenced by Tacitus's picture of Tiberius); finally, a major theme, such as Gibbon's irony, may be treated only very briefly by the inclusion of some ten examples with very little discussion (pp. 155-57).

This same general and introductory quality of the book is indicated by Jordan's tendency to treat everything from Gibbon's point of view, rather than moving outside Gibbon to establish a context, and by the frequent use of short quotations with no indication of the source. Ultimately, one feels that Gibbon's work and study as a young man have still not been assessed adequately, and it remains difficult not to share at least some of Horace Walpole's surprise at the unexpected appearance of the *Decline and Fall of the Roman Empire*.

GEORGE W. HOUSTON
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G. M. DURANT. *Britain, Rome's Most Northerly Province: A History of Roman Britain, A.D.*

43-A.D. 450. New York: St. Martin's Press. 1972. Pp. 179. \$6.95.

This book is based on the premise that Roman Britain is a "closed book to many if not most of the inhabitants of the British Isles." What is needed to rectify this situation, the author implies, is a book that is "neither intimidating nor darkly obscure." Whether the first statement is true or not I am unable to judge. But it is certainly true that Mr. Durant has written a frankly popular history of Roman Britain. I should add here that there is nothing wrong with popular history, provided that it is not bad history, any more than there is anything wrong with talented amateurs writing popular or any other kind of history. Britain, more than any other country, has produced a remarkable breed of free-lance historians. Whether Mr. Durant is one of them I do not know since he is not identified on the dust jacket. I doubt it for his book embodies two major faults. In the first place, although his book was copyrighted in Britain in 1969, his perfunctory bibliography contains nothing printed since 1963 and contains some curious items like Sir Charles Oman's *England before the Norman Conquest* (1938) and Charles W. Previte-Orton's *Shorter Cambridge Medieval History* (1952). The contents of the book also bear out the fact that he did not delve very deeply nor very recently into his subject. Second, Mr. Durant frequently indulges in statements that are needlessly speculative when they are not sheer guesswork. For example, Maximian's "unskilful, untrained sailors were no match for the seamanship of Carausius and his crews." Or after the capture of Fullofaudes in 367 (Durant says death thus confusing him with Nectaridius) the remnants of the sixth and twentieth legions "disbanded themselves in panic, or fought isolated ineffective actions in bands scattered here, there and everywhere." The above observations, however, have little to do with the audience intended by Mr. Durant. He has written a lively, readable introduction to Roman Britain that, despite its intellectual deficiencies, might well serve the purpose for which it is intended.

DONALD A. WHITE
Temple University

EVANGELOS K. CHRYSOS. *To Vyzantion kai hoi Gotthoi: Symbolē eis tēn exōterikēn politikēn tou Vyzantiou kata ton tetartov aïōna* [Byzantium and the Goths: A Study of Byzantine Foreign Policy in the 4th Century]. (Idryma Meletōn Chersonēsou tou aimou, 130.) Thessaloniki: Hetaireia Makedonikōn Spoudōn. 1972. Pp. 195.

As the title suggests, the bulk of this book is indeed a study of the whole span of relations between the Roman Empire and the Visigoths in the fourth century. Scholarly treatments of all or parts of this span already exist, but Chrysos's approach differs in being not only comprehensive but also in that it presents, from an essentially diplomatic and institutional point of view, an analysis of the application to the Visigoths of the status of *foederati*.

Chrysos scrutinizes systematically all the primary sources and the interpretations of modern scholars, sifting through the various phases of his topic and reassessing thoroughly all of their details. Predictably, he divides the topic into four main episodes, with a distinct chapter for each: Constantine the Great's dealings with the Goths and his treaty of 332; the Christianization of the Goths and the career of Ulfila (whose personal role is given much weight); the dilution of the relationship and its crisis under Valens; and the pacification under Theodosius the Great, with his treaties of 380 and 382, to the final stages of the Gothic position as *foederati* and its collapse with the career of Alaric.

There are fine points that could be disputed in Chrysos's study, but he is quite circumspect and fair in his presentation. Moreover, what gives his work its point is the set of transcendent perspectives into which he attempts to set his topic. The first of these perspectives consists of the institutional implications and development in the application of the *foederatus* status. The second is the argument of Stauffenberg and others (which Chrysos rejects) that the treaty of 332 marks a sharp new departure in imperial policy, replacing an "imperial" concept with a "universal" concept of relationships between the Empire and its barbarian neighbors. The third is the need that Chrysos perceives for relating the initiatives in Christianizing the barbarians to the broader aims of the Empire's external policy.

As a result, and above all, Chrysos seeks to present his topic as a case study in Byzantine diplomatic history and methodology. In opposing Stauffenberg's position he sees the fourth century as a transitional period of adaptation, rather than as an era of radical innovation. On the other hand, he sees this period as formative in generating the first elements of subsequent Byzantine theory concerning the "family of rulers" as presided over by the Byzantine emperor, a theory examined extensively by Dölger and other recent scholars. He concludes: "The Eastern Empire's experience with the institution of the Visigoths as *foederati* was not

forgotten. It was applied successfully in the fifth century and later it constituted the basis for the establishment of the Empire's relationships with the peoples that came to be settled in the Balkan Peninsula" (pp. 173-74).

Chrysos has produced a useful treatment of aspects of fourth-century history and of Romano-barbarian relations. At the same time he has provided a stimulating explication of the origins of what would later become basic principles of mature Byzantine diplomatic theory and practice. In so doing he has made a worthwhile contribution to illuminating that notorious, fascinating, but sadly neglected time zone in which the late antique and the early Byzantine overlap. The only regret is that the book's language will tend to limit the circulation and accessibility it deserves.

JOHN W. BARKER
University of Wisconsin,
Madison

MEDIEVAL

HELMUT MAURER. *Konstanz als ottonischer Bischofssitz: Zum selbstverständnis geistlichen Fürstentums im 10. Jahrhundert*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, 39. Studien zur Germania Sacra, 12.) Göttingen: Vandenhoeck & Ruprecht. 1973. Pp. 99, 9 plates, 1 map. DM 18.

In contrast to much current work on medieval town origins, this essay ascribes the tenth-century growth of the city of Constance entirely to its bishops' desire to glorify the Church and their Ottonian masters by reproducing the sacral and ecclesiastical topography of Rome. The careers of the three most active bishops are sketched to show how a minor bishopric, contained within the walls of a small Roman fort, had grown by the beginning of the eleventh century into a major trading town, attracting Italian merchants to its linen market, and comparable as a branch of the imperial Church to Liège or Hildesheim.

A pupil of Notker the Stammerer, Bishop Salomo III, started the program, rebuilding the minster to house a relic of the martyr Pelagius he had brought from Rome. The resulting influx of pilgrims led to the establishment of a market and a mint. Next, Bishop Konrad built an almshouse and two new churches, one of which, St. Paul's, was constructed deliberately away from the city's center to duplicate Rome's St. Paul Without the Walls. He also presented the church of St. Lawrence with a relic of its martyr, which he had obtained in Rome, and

founded a church in honor of St. Maurice. Since Otto I had ascribed his victory over the Magyars in 955 to the aid of Saints Lawrence and Maurice, Konrad was clearly expressing his loyalty to the emperor as well as accommodating a growing population. The most recherché reduplication of the Roman map was made toward the end of the century when Bishop Gerhard founded St. Peter's monastery in the swampy land on the north bank of the Rhine (providing it with a relic of Gregory the Great) to imitate St. Peter's situation in Rome, where the church is located across the Tiber from the main part of the city. Between them the three bishops had built a "new Rome" as a microcosm of the renovated Roman Empire, with five station churches strung out along a processional route, and had secured the patronage of the Ottonian house by the most judicious flattery.

BERNARD S. SMITH
Swarthmore College

PAUL DE VOOGHT. *Jacobellus de Stříbro* (†1429): *Premier théologien du hussitisme*. (Bibliothèque de la Revue d'Histoire Ecclésiastique, number 54.) Louvain: Bureaux de la R.H.E., Bibliothèque de l'Université. 1972. Pp. xv, 413. 600 fr. B.

With this study Dom De Vooght has filed a brief in defense of a successor of Hus written in much the same spirit as the author's earlier defense of Hus himself (*L'Hérésie de Jean Hus* [Louvain, 1960]). Jacobellus, who was a friend and colleague of Hus and a theological leader of the Hussites for more than a decade after Hus's death, is described by De Vooght as having remained within the boundaries of "Catholic tradition" on most points of his teaching. Like the defense of Hus, this brief for Jacobellus stays exclusively on a theological level. For De Vooght the question is not whether Jacobellus exuded a reformist or revolutionary spirit. He asks only if Jacobellus asserted theological positions that a fair-minded, congenially ecumenical judgment could call heretical.

The reader can only wonder if Jacobellus would have retained such a one as De Vooght for his advocate. That zealous spirit Kaminsky ascribed to Jacobellus, the spirit that insisted that Communion in both kinds was not merely tolerable but necessary to salvation, that Communion should be given to children, that the Roman Church was the embodiment of the anti-Christ, that immoral behavior by priests threatened the efficacy of their ministry—that spirit is not emphasized by De Vooght. In every

case the author focuses on the limits of Jacobellus's critique not on his radical thrust. In order to save Jacobellus, De Vooght postulates radical evil and heresy in other persons and then denies that Jacobellus was in league with them. Thus Wyclif is the archheretic and the Taborites the intolerable radicals. Jacobellus's distance from them is emphasized.

The splendid isolation of the theological approach becomes particularly difficult as De Vooght explicates Jacobellus's essay against usury. A discussion of merchant classes in Bohemian cities and their reaction to this issue as it reflected on their attitude toward the Hussite revolt would have been most helpful.

Even granting De Vooght his methodological presuppositions, some points are unclear. Jacobellus is lauded for adhering to "Catholic tradition," but as a matter of fact, quotations from his own writings pay obeisance to the early Church. Scripture and the early Church are used by Jacobellus to criticize that which "Catholic tradition" has handed over to the early fifteenth-century Bohemian Church. De Vooght paints an exceedingly negative picture of Gerson and his colleagues at Constance who opposed Utraquism. In fact, De Vooght seems to share Jacobellus's criticism of what had become of "Catholic tradition" by 1415. When "Catholic tradition" lost its way is unclear. Scholastics as late as Thomas are cited as respected authorities. The tradition after Thomas is more criticized than respected. Had De Vooght filed his brief in the fifteenth century rather than in the twentieth, Gerson and his circle might well have found him to be in contempt of court.

The study is done with the thoroughness and care De Vooght lavished on his earlier defense of Hus. Readers who might quarrel with the method employed by the author are rewarded with long quotations from Jacobellus's writing including critical editions of three essays in the appendix. The work is another landmark venture, offered from the Catholic side, to aid the search for ecumenical rapprochement. No student of the development of the Hussite movement can neglect it.

PAUL L. NYHUS
Bowdoin College

DAVID JACOBY. *La féodalité en Grèce médiévale. Les "Assises de Romanie": Sources, application et diffusion*. (École Pratique des Hautes Études, VI^e Section. Documents et recherches sur l'économie des pays byzantins, islamiques et slaves et leur relations commerciales au Moyen Âge,

number 10.) Paris: Mouton & Co. 1971. Pp. 358. 72 fr.

With the occupation of Constantinople in 1204 by the armies of the Fourth Crusade and the establishment of the Latin Empire, vast territories of the Greek mainland and the Aegean archipelago—the area called Romania—came under Western, mainly French and Venetian, influence. An immediate impact of the Latin domination was the introduction of Western-type feudal institutions and the development of a feudal law in the area.

Professor Jacoby, from the Hebraic University of Jerusalem, in his exhaustive and richly detailed study examines the origins, formative elements, and evolution of the feudal law of Frankish Morea, as compiled in the *Assizes of Romania*, giving full and detailed account of the application and recognition of the assizes in various other parts of Frankish Greece and in the Venetian possessions of the Levant. The book is essentially legal history, and in this field it will certainly remain the most comprehensive and indispensable work of reference for a long time. Citing numerous individual legal cases and analyzing the relevant articles of the assizes, the author demonstrates the complex nature of the law of Romania that blended Western feudal concepts with local Greek customs and Byzantine legal elements. Through adaptation in the Venetian possessions, it was also influenced by the more centralistic judiciary system of the Republic of Venice.

On the basis of certain historical facts and names mentioned in the assizes, the author places the time of the compilation of the first complete French text of the *Assizes of Romania* in the time period between 1333 and 1346, when the principality of Morea already passed under the suzerainty of the House of the Angevins of Naples. The original French text was lost; an Italian translation in Venetian dialect was prepared and approved in 1421 by the chancery of the doge of Venice as authentic text (*autenticum cancellariae*). Eventually, in 1453 and after the addition of thirty-seven new articles, the Senate of Venice declared this official text of the assizes as a law in force in all Venetian possessions of the area.

The Venetian version survived in twelve manuscript copies dating from various periods between 1423 and the second half of the eighteenth century. Professor Jacoby analyzes each manuscript separately, and in discussing the application and recognition of the assizes in the Peloponnesus and in the Venetian posses-

sions of the Aegean, he includes a great number of individual case histories. They refer to the personal status of noblemen, litigations over proprietary and feudal rights, questions of inheritance, and, besides their legal aspects, offer interesting insights into the complex social structure and the political and economic conditions of medieval Greece. As appendix, twenty unpublished documents in Latin from the State Archives of Venice complete the book.

Professor Jacoby's work takes full account of the voluminous primary and secondary sources and reveals a fascinating and lesser-frequented area and period of medieval history. His technique of accumulating a great amount of detail material makes it sometimes difficult for the uninitiated reader to piece together a coherent picture of the feudal society of medieval Greece; the academic scholar, who is interested in the study of the period, will find in the book an invaluable source of information.

ANDREW URBANSKY

University of Bridgeport

CONSTANCE HEAD. *Justinian II of Byzantium*. Madison: University of Wisconsin Press. 1972. Pp. xi, 181. \$10.00.

Professor Head has two primary aims in her recent book on Justinian II: to reinterpret his reign in a way that stresses its accomplishments, and to write a work that will be readable by nonspecialists.

In the second aim she succeeds admirably. This is a well-written book, easily accessible to readers who have an interest in Byzantine history without being specialists in the field. This is a virtue that is rare enough in historical works, and perhaps rarer still in the Byzantine field. It does entail certain problems. For example, the treatment of the thorny problems of the themes, the village communities, and fiscal reforms (chapters 11 and 12) is so summary as to be almost superficial. The Slavic settlements in the Byzantine Empire are never discussed sufficiently; Professor Head speaks of "Sklavinia" in such general terms that the non-specialist may well wonder whether this was a defined area and if so where it was situated and what its extent was. These problems stem from the fact that the book tries to rehabilitate Justinian II, and so the primary interest is with the man rather than with the issues.

The first aim, that is, the reinterpretation of the reign of Justinian II, is the one that presents the most interest for the specialist. In fact, Justinian II has already been partially

rehabilitated. The first part of his reign (685–95) is generally considered to have been a productive and successful one. In what concerns this decade, Professor Head discusses achievements about which there is no longer much debate; of course, a service is rendered by the fact that the record is here presented in a unified and detailed fashion. Much more debatable are Justinian's activities after his return to the throne in 705. Professor Head tries to show his "constructive statesmanship and genuine concern for his empire" (p. 115) and contrasts it to the general impression that Justinian's second reign was marked primarily by acts of revenge against his enemies. But the only area where it could even be argued that Justinian was engaged in statesmanship was the area of foreign policy. Here one can hardly be persuaded that his fruitless quest for foreign alliances is proof of any great statesmanship. The discussion of Justinian's expedition against Ravenna is convincing in its claim that the campaign was an issue of policy and not merely an act of vengeance. But on the whole, the reader will be justified in considering Justinian's second reign as a series of unmitigated disasters.

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Brandeis University

DONALD M. NICOL. *The Last Centuries of Byzantium, 1261–1453*. New York: St. Martin's Press. 1972. Pp. xii, 481. \$14.95.

With the rapid advance of Byzantine scholarship, detailed monographs have in the last decade or so been written on the reigns of all the Paleologan emperors except for John V (which was partially dealt with by Halecki). Nevertheless, since the general histories of Vasiliev and Ostrogorsky, no one has attempted to write what Nicol himself refers to as "a more extended work of synthesis covering the period 1261–1453." Nicol now offers us such a narrative of this fascinating period of Byzantium's last two centuries when the Empire was in full decline, suffering the longest death throes of any state in historical annals. And yet during this period, Byzantium underwent some of the most remarkable social, intellectual, religious, and even political changes or modifications in her long history. In this vast panorama of development, the author has chosen to limit himself to the political sphere and, secondarily, the ecclesiastical. Perforce, therefore, he can treat only cursorily such problems as the causes and effects of the Paleologan Renaissance, discussing other cultural and social problems only as

they impinge on the political and ecclesiastical.

Nicol's work is constructed in straight chronological fashion. As may be seen from the documentation, he draws heavily upon secondary sources, very few indeed escaping his sharp eye. Although on occasion he cites primary sources to emphasize or elaborate a point (especially for the reigns of Andronicus II and John V), there is no startlingly original research entailed. The extremely complex series of events is kept in focus by the attractiveness of the narrative style and the skillful organization of the vast material. A reader might take issue with certain emphases or interpretations: despite the avowed emphasis on politics and religion, one could argue that a fuller understanding of the vital cultural and ideological differences separating Greeks and Latins in this period—when the West, after centuries of cultural lag, had finally caught up with and even, in some ways, surpassed the East—would have enhanced the usefulness of the work even more. The unionist councils of Lyons and Florence in particular might have received fuller treatment, especially as to the reasons most Greeks were so fearful, almost paranoid, regarding religious union with Rome.

Nicol is probably right in affirming that Bessarion and Cydones were "in love with Italy before they went there." One might equally stress their preoccupation with the concept of the restoration of the ancient unity of Christendom (compare Loenertz on this). Nicol also perceptively emphasizes the paradox that, while the Turkish peril affected the Byzantines to the point of their becoming "a nervous state," Byzantium, nevertheless, in its wretched condition, was thereby rendered "more fertile in ideas than in the previous two centuries."

A few points or events the treatment of which the reviewer might take issue with: the account of Michael VIII's capture of Constantinople in 1261 might take into greater account the disagreement of the sources and the factor of a possible ruse on Michael's part. Scholars now accept that the *Directorium* was written rather by Guillaume d'Adam. Greater use might have been made of Weiss's (recent) work on John Cantacuzene's reign, of Vacalopoulos's recent book, of articles of Anastasiou on Athos and Michael VIII, and of Cirac on religious union. More information on Alexander V, the Greek little-known pope, could have come from Syropoulos and other sources. But these are minor points that do not detract from the overall value of the work. Particularly well discussed are the fluctuating fortunes of

Epirus and Thessaly, the vital point that not all Palamites were pro-Cantacuzene, the surprising increase in the Orthodox world of the patriarch's authority in sharp contrast to the declining power of the emperor, and the author's (partial) rehabilitation of Andronicus II from a virtual "do nothing" historical status. In sum this volume, though offering no really new conclusions, with certain minor qualifications may be said to provide the first synthetic, unified, and well-written narrative of the long and complex Paleologan era. As such it constitutes a highly useful, learned, and admirably detailed delineation of the political and ecclesiastical events of the epoch—especially for those who, unable to read foreign languages, cannot plow through the maze of events in the minutely detailed monographs on the reigns of the many Paleologan emperors.

DENO GEANAKOPOLOS
Yale University

W. MONTGOMERY WATT. *The Influence of Islam on Medieval Europe*. (Islamic Surveys 9.) Edinburgh: University Press; distrib. by Aldine-Atherton, Chicago. 1972. Pp. viii, 125. \$5.50.

W. MONTGOMERY WATT. *The Formative Period of Islamic Thought*. Edinburgh: University Press; distrib. by Aldine Publishing Company, Chicago. 1973. Pp. vi, 424. \$12.50.

The Influence of Islam on Medieval Europe comprises a series of lectures delivered at the Collège de France in December 1970. There are eighty-four pages of text, a glossary of English words derived from Arabic, notes, and an index. The lectures make no pretense of turning over new ground but simply review the evidence from the point of view of an Islamicist. Watt is a skillful and accomplished lecturer and so the results are exactly what the series of Islamic surveys was designed to give, an enlightening presentation for the educated reader.

This has been done before, notably in the original edition (Oxford, 1931) of *The Legacy of Islam*, but that work was the product of many hands, as will be its soon-to-appear second edition. The patient reader might well choose to wait upon that latter work, but in the meantime any student of the subject will be clearly and succinctly instructed and guided by Watt's notes directly into the scholarly foundations upon which these lectures so gracefully rest.

The Formative Period of Islamic Thought is a work of primary scholarship; it is an important study of an important and not much

understood period in the intellectual history of Islam. The book is based, as Watt says, upon a "radical critique" of the sources of Islamic theology up to about 950 A.D. and attempts to present the results of that critique in the form of an evolutionary history of the period. Watt is generally kind to his readers, and here he provides (pp. 1-6) a forthright set of principles upon which he intends to proceed. The need for such flows from the fact that the finished version of the history of Islamic theology was written by theologians whose self-constituted "orthodoxy" had won general acceptance and whose treatment of their predecessors, the thinkers of Watt's "formative period," was chiefly in the form of heresiography.

The results of Watt's "radical critique" is a rich and provocative book, which is the first attempt to explain early Islamic theology on such a broad scale. But one must conclude almost immediately that the heresiographers have not entirely yielded up their secrets. Information still holds its pre-eminence over sense, and many of the heresiographers' lists of names and positions continue to clog the pages of Watt's work without yielding much understanding. Theology still stands isolated from the political, social, and economic evolution of Islam.

Watt is a careful and conservative scholar, and much of his analysis is convincing. Where he fails, I think, is in his scanting of the metaphysics of Jahm ibn Ṣafwān, his ignoring of the *ghulāt* or "extremist" elements in the early history of Shi'ism, his silence on early Shi'ite *kalām*, and a totally inadequate account of the origins of Ismā'ilism.

But there is an issue of somewhat larger substance between us. Watt is forthright in conceding that the "Islamic thought" of his title is really Islamic theology. But the exclusion, with simply a passing notice, of other intellectual currents is difficult to defend. The period covered by this book is precisely the time when Muslim life was exposed to great drafts of Hellenic occultism, scholasticism, and science, all of which deeply affected Muslim thought. Watt does speak from time to time of "Greek ideas" or "Greek methods of argument," but he never says exactly what they were, where they came from, or what effect they had on Islam. The omission here is serious, and Watt's pages on "the attraction of reasoning" are among the least successful in the book.

These are simply preliminary reflections. Watt's *Formative Period* occupies new ground, and only after it has been thoroughly studied

and tested against its own sources will one be able to say how much of it deserves to stand there. For others less specialized, it may well be an interesting example of a discipline emerging from almost utter incoherence into the first outlines of a *communis opinio*.

F. E. PETERS

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DEREK BAKER, editor. *Relations between East and West in the Middle Ages*. Edinburgh: Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1973. Pp. 156. \$8.00.

This book, consisting of papers presented at a colloquium at the University of Edinburgh in March 1969, is an excellent accomplishment. This is true despite a somewhat misleading title. The East is in fact the "eastern part of the Graeco-Roman world," with heavy concentration on the Byzantine Empire and with emphasis almost entirely on the dynastic, ecclesiastical, and cultural. The one paper that deals at all with the economic and institutional is limited to Latin Syria. The book is really a collection of monographs (the second section by Karl Leyser, "The Tenth Century in Byzantine-Western Relationships," runs to 19 pages and is accompanied by 12 pages of notes in fine print).

This reviewer is not competent to assess the value of the papers by W. H. C. Frend, "Old and New Rome in the Age of Justinian," and that by Karl Leyser, but I find new material and new interpretations in the papers by R. H. C. Davis, "William of Tyre"; by Anthony Bryer, "Cultural Relations of East and West in the Twelfth Century"; and by Joseph Gill, "Innocent III and the Greeks: Aggressor or Apostle?" The paper by Jonathan Riley-Smith, "Government in Latin Syria and the Commercial Privileges of Foreign Merchants," is full of new approaches to the subject and a mass of information on tolls, taxes, and the *funda*, information not available elsewhere except in the relevant sources, which he seems to have examined with remarkable thoroughness. The general scholarly reader will perhaps find the papers on "William of Tyre" and the concluding paper by R. W. Southern, "Dante and Islam," especially interesting and certainly more readable. The former presents the thesis that William of Tyre's *History Overseas* was a "message" his readers never recognized; they read it as an adventure story and he had meant to warn them that Jerusalem could be saved only if the Muslims were

divided and the Christians united. Southern has an interesting hypothesis concerning the place in the *Divine Comedy* of Mohammed and his son-in-law Ali. He concludes his paper stating that "a few western travellers of Dante's day knew Islam at first-hand and brought back a sympathetic understanding of the Muslim way of life, but Dante was not one of them. He was a wholly western man."

There is an excellent and very helpful ten-page introduction by Donald M. Nicol. And, marvel of marvels for this type of book, there is an excellent index.

SIDNEY R. PACKARD
Smith College

FRIEDRICH PRINZ. *Klerus und Krieg im früheren Mittelalter: Untersuchungen zur Rolle der Kirche beim Aufbau der Königsherrschaft*. (Monographien zur Geschichte des Mittelalters, number 2.) Stuttgart: Anton Hiersemann. 1971. Pp. xxiv, 216. DM 60.

This important study traces the disparity between the officially stated Church position on the participation of the clergy in war and the actual practice of the clergy in military activities between the fifth and the tenth centuries. It is Prinz's thesis that changing societal, political, and military factors in Western Europe, especially the decline of Rome, compelled the Church to accommodate itself to new situations.

Beginning with an examination of the decretals of the fourth-century councils, Prinz shows that all clerical participation in warring activities was forbidden, even the bearing of arms. Yet, as early as the late fifth century, bishops such as Germanus of Auxerre, Hilarius of Arles, and Pope Leo I himself were already involved, by will or by circumstance, in political affairs that carried military responsibilities. An increase in the civil power of the bishops, coupled with the fact that the higher clergy were increasingly of noble background—a process the author aptly calls the "aristocratization" of the clergy—led to the justification of their participation in military functions from scripture and patristic literature. This is all solidly documented with examples from Gallo-Roman territories and in Merovingian times.

This evolving development was accelerated or "institutionalized" by Charlemagne who, in making the higher clergy integral parts of the imperial aristocracy, required them to produce fixed contingents of armed knights for his service. Evidence abounds of clerics participat-

ing in Charlemagne's many campaigns, and imperial theologians rationalized this state of affairs by equating resistance to the king with resistance to God. The disintegration of the Carolingian Empire and the international crisis that followed it inexorably led to the increased military responsibility of the higher clergy, many of whom led contingents against the invading Normans and Hungarians. The late tenth century vita of Archbishop Bruno of Cologne, written by the monk Ruotger, depicts him as a model ecclesiastic, a "good shepherd," although Bruno violated every canon of clerical nonparticipation, which illustrates the drastic alterations the old ideals had undergone. By the time of Gratian's *Decretum* in the middle of the twelfth century, the development of the crusading ideal and the partial militarization of the papacy had brought the relationship of the cleric and war into a new ambient.

While much of this has been known or suspected since the studies of Kleinclausz and Lot, Prinz's contribution is in his careful examination of the social background of the Merovingian and Carolingian episcopates. The book is a soundly researched and authoritative work that students of many areas of early medieval history will want to consult.

BENNETT D. HILL
University of Illinois,
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STANLEY CHODOROW. *Christian Political Theory and Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum*. (Publications of the Center for Medieval and Renaissance Studies, UCLA, number 5.) Berkeley and Los Angeles: University of California Press, 1972. Pp. xi, 300. \$15.00.

Basically this book expounds Gratian's concept of the constitution of the Church as embodied in his *Decretum*, a dialectical legal treatise completed shortly after 1139. Although the work became the standard medieval textbook for canon law, it was studied by scholastic lawyers not for the conclusions of its compiler, but for the multitude of sources he had collected there, from which later canonists drew different conclusions. Modern historians have studied Gratian's own opinions on many particular points, but Chodorow is the first to attempt a general view of the master's ecclesiology.

Necessarily the bulk of this work is descriptive, and the detailed explication of Gratian's doctrine will certainly be welcome grist to the

mill of specialists in medieval political and legal thought. They should particularly appreciate the author's incisive ability to express complex ideas in simple and precise language. I particularly admired the lucid topical organization, which might well be taken as a model for similar studies of other thinkers. Indeed the clarity of this presentation gives it a special value as an introduction to the categories and concepts of medieval political thought.

Chodorow, however, is no mere expositor. He maintains the bold and original thesis that Gratian was the theorist of a post-Gregorian reform party, the typical member of which was Bernard of Clairvaux. The nucleus of this party, which unquestionably did exist during the schism of 1130, was a curial faction headed by the French cardinal Haimeric, papal chancellor 1123-41. A generation ago Hans Klewitz argued that the party flourished throughout Haimeric's chancellorship and that it favored a shift from the old Gregorian stress on the temporal welfare of the Church to a new program of spiritual regeneration. This position was later elaborated by F. J. Schmale, who found that the faction at the *Curia* consisted of cardinals from France and northern Italy, and its supporters in the provinces were monks and canons regular. Against this proposal Gerd Tellenbach objected that the ideology of the party was not sufficiently distinct from that of its opponents. That deficiency Chodorow now attempts to supply by taking Gratian as the exponent of the party's ecclesiology, though he realizes that Tellenbach's objection cannot be wholly overcome as long as the position of the opposing party remains unknown on crucial issues (p. 246). Nonetheless the author is able to draw repeated and striking parallels between the thought of Gratian and Haimeric's supposed partisans, notably Bernard. These discoveries, I think, will be the seedbed for a fruitful reappraisal of the development and significance of twelfth-century political theology, but in the process I suspect that it will prove wiser to discard as anachronistic the conceptual model of organized political parties with ideologically based programs.

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PERCY ERNST SCHRAMM. *Kaiser, Könige und Päpste: Gesammelte Aufsätze zur Geschichte des Mittelalters*. Volume 4. Part 1, *Rom und Kaiser; Geistliche und weltliche Gewalt; Das*

Reformpapsttum; Zur Geschichte von Nord- und Westeuropa; part 2, *Zur Geschichte von Süd-, Südost- und Osteuropa; Zusammenfassende Betrachtungen*. (Beiträge zur allgemeinen Geschichte.) Stuttgart: Anton Hiersemann. 1970; 1971. Pp. 432; 439-764. DM 170 the set.

With this volume, completed just before his death in November 1970 (the foreword is dated July 5, 1970), Schramm was able to realize what he must have conceived to be the major goal of his *Gesammelte Aufsätze*: the presentation in an organized fashion and in an updated version of his massive and diverse writings on the theme of *Herrschaftszeichen, Staatssymbolik*, and *Staatsrepräsentation* in the Middle Ages. Whether any of the plans Schramm outlines for future publications (pp. 6-8, 727-33) are realized posthumously, all medievalists can be grateful that they now possess Schramm's final and full position on the broad subject to which his contribution over five decades has been so seminal.

This volume, published in two parts, makes little sense by itself. It can only be judged as a continuation and completion of the three previous volumes (reviewed in *AHR*, 75 [1969]: 462-63, and 77 [1972]: 127-28). In general the substance is somewhat thinner and more diffuse than that of the earlier volumes, chiefly because Schramm in his effort to present all that he has written on his basic theme moves somewhat far afield from those topics, geographical areas, and eras to which he has devoted his most thorough investigations. A brief outline of the contents of this volume will serve best to indicate what Schramm has done to complete his collection of studies relating to the signs, symbols, and rituals utilized to represent the offices of medieval kings, emperors, and popes.

The first three sections (pp. 19-203) comprise a miscellany of studies on familiar themes: texts concerning Rome and the emperor in the High Middle Ages, aspects of the relationships between "geistliche und weltliche Gewalt," and the ideology of the reforming papacy of the eleventh and twelfth centuries. The major items include a study of eleventh- and twelfth-century Roman literature on the topography and history of ancient Rome (pp. 22-33); a study aimed at elucidating the *Dictatus papae* by tracing the history of how certain prerogatives of *sacerdotium* and *regnum* became intertwined to create key elements of that document (pp. 57-102); some remarks on the history of the papal tiara (pp. 107-12); a study of the "throne of the popes" in St.

Peter's (pp. 113-22); an analysis of the Old and the New Testaments "in der Staatslehre und Staatssymbolik des Mittelalters" (pp. 123-40); and a study of the two fragments of Cardinal Humbert's *De sancta Romana Ecclesia* viewed as a programmatic statement of the reforming papacy of the eleventh century (pp. 143-64). All of these studies except that on the throne of the popes are revised versions of earlier publications. Interspersed among them are assorted shorter items, chiefly book reviews, related to the major themes.

The fourth section, comprising the largest segment of this volume (pp. 207-631), is devoted to about forty items treating the broad theme of *Herrschaftszeichen und Staatssymbolik* in "Länder" other than the Holy Roman Empire: the Scandinavian kingdoms, England, France, the Spanish kingdoms, Italy, the crusading states, Hungary, Bohemia, Poland, and Russia. Although conceding a lack of expertise with respect to the histories of these many realms, I feel fairly confident in suggesting that Schramm fails to achieve his usual profundity, intimate contact with the sources, and insight into the fundamental nature of medieval political authority. Consisting chiefly of book reviews and papers presented to conferences, these studies do not effectively convey what one suspects Schramm wished to convey: a sense of the common elements and the local variations associated with the symbols, rituals, and forms utilized to express the political concepts of royal and ecclesiastical offices in the entire medieval world. What Schramm presents, however, suggests that a treatment in depth of this theme on a pan-European scale would be extremely fruitful; as he indicates in his foreword, Schramm had such a plan in mind for himself and his students (p. 7).

The fifth section (pp. 634-733), entitled "Zusammenfassende Betrachtungen," is frustrating. If the reader anticipates a master historian's overarching recapitulation on a theme so massively treated in four volumes and in other equally massive books, he will be disappointed. He will find a strange un-integrated collection of book reviews, unpublished papers, and extracts from previous volumes touching on historical research in general, art history, and "Geistesgeschichte," on Schramm's basic approach to the study of the signs and symbols through which medieval political concepts were expressed, on the matter of Europe and its nations, and on what constitutes the common characteristic of the

Middle Ages. Yet every page presents a stimulating idea that makes one realize the profound insights that Schramm developed during his long career with respect to medieval civilization, history as a discipline, and the relationship between the past and the present.

In the foreword to this volume, Schramm wrote: "Ich schrieb viel, aber es ist Stückwerk geblieben." I would like to think that this statement absolves him of any obligation to make a meaningful summation of what he has achieved in the first four volumes of the *Gesammelte Aufsätze* that he obviously wished to be judged as an entity. In honesty, there can be no absolution; these volumes beg for a meaningful evaluation that would provide a key to Schramm's contribution to the study of the Middle Ages. This task, however, lies beyond my competence. Schramm's collection is too varied, too rich in detail, too innovative in approach and methodology, and too profound in implication to permit a yeoman medievalist to comprehend its full significance. Perhaps what is needed is a symposium of scholars from many branches of medieval studies who would focus their varied perspectives on Schramm's total scholarly production in order to render final judgment on what this great scholar has achieved. Such a collective evaluation would, I am certain, conclude that all medievalists are indebted to Schramm for charting a new access route to the medieval world, for delineating the rich meaning of innumerable signposts along that route, and for pointing toward unexplored territory still beyond. If this be "Stückwerk," more of the same kind would be a great gift to all medievalists.

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HELMUT COING, editor. *Handbuch der Quellen und Literatur der neueren europäischen Privatrechtsgeschichte*. Volume 1, *Mittelalter (1100-1500): Die gelehrten Rechte und die Gesetzgebung*. (Veröffentlichung des Max-Planck-Instituts für europäische Rechtsgeschichte.) Munich: C. B. Beck'sche Verlagsbuchhandlung. 1973. Pp. xxiv, 911. DM 118.

The first thing that impresses the reader is the courage of the editor in listing and discussing the sources and literature of medieval "private law" in one volume, however formidable. But Coing seems to have been well aware of the risks, and he presents the limits of his enterprise very systematically (pp. 3-15). The main

purpose of the handbook is to serve as an introduction to the civil law of modern Europe, which will be treated in the three volumes to follow. It was necessary to include the later Middle Ages because that was the period when "the foundations were laid for a unity of European law that led to the system of Civil Law proper" (p. 3). These roots of later development are seen in the study of law at the universities, the spread of legal scholarship, and the emergence of a particular group of university-trained lawyers. In this spirit the volume concentrates on the intellectual and institutional developments on the Continent from the twelfth to the fifteenth century, while England on the one hand and the Byzantine-Slavic world on the other are adduced only as comparisons. Only those laws, statutes, and commentaries are discussed that were more or less touched by the impact of revived legal studies: legalistic and canonistic sources and literature, learned commentaries on territorial laws, literature on civil legal procedure, the legislation of later medieval principalities (including the Empire), and the jurisdictional practice of the Rota Romana. Wisely, no attempt was made at a rigid distinction between "public" and "private" law, which would have been anachronistic for the Middle Ages; actually, a whole section is devoted to an overview of royal, ecclesiastic, and corporate institutions (including the public notariate) of medieval Europe (pp. 401-514). Even with all this sensible limitation, the editor and his eight colleagues (Gero Dolezalek, Gunther Gudian, Norbert Horn, K. W. Nörr, Hansjörg Pohlmann, Winfried Trusen, Peter Weimar, and Armin Wolf) had to cover an immense field of primary and secondary material. It is inevitable that some chapters are better than others. For example, the section on faculties of law, which logically opens the book, begins with a survey of all medieval universities—their history, structure, and legal status. Although this is a very useful and handy guide to which nothing similar exists, it leads somewhat beyond the scope of the handbook. On the other hand, medieval society and economy, hardly a neglectable element in the development of civil law, are each taken care of by a page with haphazardly chosen, very meager bibliographies. The sections on legal matters proper are less uneven, and, if so, for good reason: where up-to-date introductory works are available, for example, on canon law, the handbook can be summary, but where even the basic systematic research is wanting, for

example, on medieval legislation, the authors correctly go into detail.

Besides the excellent lists of sources—many of which are only available in early prints, hence not easily located—and modern literature, every chapter summarizes the present state of research and suggests problems worth studying. There seems to be complete agreement among the authors that the older, somewhat mechanistic notion of “reception of Roman Law,” and, even more—as it was sometimes put—the idea of “struggle” between the learned laws and traditional custom, need to be basically revised. The authors see the process rather as a part of the intellectual revival of the twelfth century, as an element in the increasingly scientific view of the world in which legal studies added a dimension of new ways of thinking and writing. They also point out that Roman law served everywhere—even, in England, where it has not been “received”—as a “gold mine of ideas and patterns” (H. U. Kantorowicz) that complemented local and traditional laws for solving the questions of a changing society.

Finally, a sincere compliment is due to the contributors for their veritably pan-European, comparative approach: without clouding the differences between the various territories and communities of Europe, they present the major lines of common development with characteristic examples from all parts of the Continent. The Max Planck Institute for European Legal History has done a great service in sponsoring this book and has presented a valuable example of successful teamwork.

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JOHN H. A. MUNRO. *Wool, Cloth, and Gold: The Struggle for Bullion in the Anglo-Burgundian Trade, 1340-1478*. Toronto: University of Toronto Press. 1972. Pp. xi, 241. \$20.00.

This excellent study can in some respects be regarded as a sequel to Dr. Thielemans's *Bourgogne et Angleterre: relations politiques et économiques entre les Pays-Bas bourguignons et l'Angleterre, 1435-1467* (1966), and is itself complemented by Dr. Spufford's *Monetary Problems and Policies in the Burgundian Netherlands, 1433-1496* (1970). Thanks to these three books we can at last really begin to understand how economic and monetary affairs shaped the policies of the Valois dukes of Burgundy and affected the course of events in and around their possessions in the Low Countries.

Munro writes without great elegance of style

but in a pleasant, direct, and straightforward way that enables him to explain the technicalities of a difficult subject with consistent success. He supports his exact scholarship with a copious documentation culled from the archives of the Burgundian mints and other muniments. The only predecessor in English of this well-founded and original work is L. V. D. Owens's thin and ragged narrative *The Connection between England and Burgundy during the First Half of the Fifteenth Century* (1909). What astonishing progress in historical scholarship has been made between these two works!

Wool, Cloth, and Gold adds a new, hitherto almost unexplored, monetary and economic dimension to our knowledge of the diplomatic relations of England and Burgundy. It takes an altogether new look at the relations of Richard II of England and Duke Phillip the Bold. It investigates the successive though short-lived Burgundian bans on imported English cloths of 1434, 1447, and 1464 and their effects. It shows how the first of these in particular was not so much protectionist as retaliatory, for it was a countermeasure to English bullionism. Especially notable are the author's perceptive discussions of this protomercantilism, and of other contemporary economic and monetary theories and practices. He goes a long way to describe and explain the transformation of the English and Low Countries industries in the later Middle Ages.

We may hope that much more of the complex story of the decline of English wool exports and of the great Flemish cloth-manufacturing towns, and of the rise of the English cloth industry and of the “new draperies” in the Low Countries, will be elucidated in the author's promised forthcoming volume. He has made an admirable start in this one.

RICHARD VAUGHAN
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NICHOLAS ORME. *English Schools in the Middle Ages*. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1973. Pp. xiv, 369. \$25.00.

This interesting study of English schools in the Middle Ages extends from the twelfth to the mid-sixteenth century. In it the author draws attention to his predecessors in the field and particularly to the merits and shortcomings of the chief of these, A. F. Leach. His concern in the present work is with “Education and Society,” “Schools and Their Studies,” and “Historical Developments.” He considers vari-

ous schools, conditions in which they developed, kinds of people and the variety of interests they served. The most prominent and widespread schools were "public and secular," that is, "'public'" or "not confined to any particular class of persons" but "open to all who could afford to attend them," and "'secular'" meaning they were staffed and attended "by secular priests or clerks, and later on laymen too," rather than by members of religious orders, which maintained their own schools. The author further draws attention to the effects on the schools of the separation from Rome and the Reformation.

According to their studies the schools were song or primary, grammar or secondary, and schools of higher learning. In the primary or song schools were taught the alphabet, basic prayers, and reading in church service books, such as the psalter and the mattins book, and plainsong, possibly also writing. The grammar or second tier schools, not necessarily separated from the song schools, concentrated on the teaching of the Latin language and literature, with special attention to the structure of language and literature. Here we are reminded that the author's sources are drawn largely from the later Middle Ages and may reflect the influence of humanism. Opportunities were also provided for more practical instruction in dictamen, the art of letter writing, keeping accounts, preparing deeds and charters, as well as for studies in French and, after 1349, in English. Higher studies, that is, the liberal arts and theology, were usually pursued in the secular cathedral schools and in the schools maintained by the regular orders. For all the schools treated Dr. Orme provides details regarding the texts used, as well as information relating to their management, daily routine, and the various methods of their support and maintenance. And he follows this by an account of their development historically as to numbers, endowments, and the interest they aroused among their contemporaries. He does not, however, consider the trade schools, Inns of Court, or universities.

Throughout the work Dr. Orme supports his discussion with specific examples and citations drawn from contemporary sources. He also notes that further elucidation of the actual content of the curriculum and definite conclusions as to the extent of literacy must await further painstaking research in the mass of extant literary material available, especially for the earlier end of the chronological scale. In the meantime, we are grateful for the present

study illuminated by photographic illustrations and maps showing the numbers and locations of the schools, together with the detailed listing of all known medieval schools. A bibliography and index complete the study.

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W. L. WARREN. *Henry II*. Berkeley and Los Angeles: University of California Press. 1973. Pp. 693. \$20.00.

Henry II is one of England's great kings. For thirty-five years he dominated not only his kingdom and its dependencies in the British Isles but also most of western France. England, which gave him his royal dignity, he won by force of arms from his cousin, Stephen. Normandy, with its claims to hegemony over Brittany, his father had won for him, while greater Anjou, which included Maine and the Touraine, he inherited on his father's death, and Aquitaine was his wife's duchy. His "Angevin empire," as modern scholars have dubbed it, was never a real unit, but rather a congeries of separate dominions. As Mr. Warren makes plain in this excellent study of Henry and his reign, a lesser man would have found it impossible to meet the challenges of all these diverse polities. To keep order over so large an area when the fastest method of communication was by horse and the laws were more often than not observed in the breach, to foster peace and maintain tranquillity when force and violence were so common, was to undertake the improbable if not the absolutely impossible. What made Henry great was his ability to realize all his rights and dignities, to keep law and order, and even more, to legislate and to administer innovatively, to put the stamp of his will on the intractable material of his lordships. Clearly he was in the great tradition of the Conqueror, of Henry I, and looking to the future, of Edward I.

How could Henry achieve so much? In the first place, he was very careful to collect all the revenue he could, though not like Richard willing to sell anything at all if he could get a good price. Mr. Warren makes clear how concerned Henry was with revenue, but he does not give us much in the way of figures; quantitative data do not interest him, it would seem. Rather he is more concerned with Henry's use of law as a means to political ends and in its development in England almost as an end in itself. Here he has made a major contribution

and whoever reads this book will understand better the development of the common law. With money at his disposal and law on his side, Henry could employ mercenary soldiers as a police force to keep order and if necessary, destroy the castles of any rebels in all his far-flung dominions. Henry was lenient to rebels but not to their castles, which he dismantled at a great rate. Thus Henry reduced the power of his magnates and rejoining their power to his own authority. He was, of course, creating the precedents for the Capetians to use against his sons in order to subordinate the Angevin empire to the French kingdom. In his own time, indeed, his achievements nearly foundered on the rocks of clerical privilege and of filial insubordination. The longest single section of the book deals with Henry and the Church, centered inevitably on the Becket controversy. The account is enlightening in stressing Becket's habit of making grand gestures and Henry's flexibility, learned during the controversy, so that even after the martyrdom he lost little of what he wanted from the Church. Henry's last years were dominated by the family quarrels that are all too well known. Mr. Warren, one suspects, was a bit bored with the subject and this part of his book is a little less satisfying than the rest. Still, he shows how Henry won time and again until he became too sick in body and spirit to fight any longer.

What manner of man was Henry II to accomplish all that he did? He bemused his contemporaries because he seemed to love paradox, to say things that belied his actions, to do things that belied his words. Even to Mr. Warren he remains a bit of an enigma because Henry kept his own counsel. But everyone agrees on the force of Henry's personality, on his incredible energy, his restlessness, his seriousness, and above all, his intelligence. He was courteous, affable, even amiable, if he wished to be, but he could also be very caustic and insulting. His wrath was memorable; his laughter was ready and was as likely to be directed at himself as others; his love was given slowly but he was equally slow to withdraw it; his grief was great. But rarely did he allow his emotions to overrule his thoughts. His court was too sober and serious to appeal to chivalric youths such as his older sons and their friends. His recreations were hunting and reading. He abhorred war and could only be called the aggressor in three of his many campaigns, those coming early in his reign. Mr. Warren shows Henry changing over the course of his life from inflexible youth determined to recover all his inheritance, to flexible maturity attempting to

preserve his lordships. He shows Henry's policies reflecting his advisers. But there is fortunately none of the fashionable Freudianism of psychohistory, and the profitless speculations of Anouilh's *Becket* or Goldman's *Lion in Winter* are carefully eschewed. Indeed, Mr. Warren is so little interested in family relationships that his genealogy is not always quite correct. We will learn little here about either hereditary or environmental influence on the formation of Henry's personality. What we will learn is the character of the great king.

This is by all odds the fullest as well as the best study of the life and reign of Henry II yet written. The text amounts to 630 pages aside from twenty-six full page plates and seventy pages of prefatory material, appendix, bibliography, and index. Since the bulk is so great, some potential readers may be intimidated, and it may be worthwhile to say that Callimachus's judgment about big books being big evils does not apply to this one. Mr. Warren knows how to write to maintain his readers' interest. He has something to say and says it well. Altogether his is a splendid addition to what is already a distinguished series.

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JOEL THOMAS ROSENTHAL. *The Training of an Elite Group: English Bishops in the Fifteenth Century*. (Transactions of the American Philosophical Society, New Series, volume 60, part 5.) Philadelphia: the Society. 1970. Pp. 54. \$2.50.

The election of bishops—in the original sense of the term: their choosing by whatever means—is in any age a central fact for the ecclesiastical historian, and in virtually any medieval context for political and social historians as well. For the former, even if he holds the loftiest views of episcopacy it is clear that the choice of the Holy Spirit is more likely to fall on some than on others; for the latter, patterns of influence and currents of elitism displayed in episcopal choices can illuminate matters far wider than mere church politics.

A commonly held belief about the fifteenth-century English episcopate is that it was both predominantly aristocratic and skilled in government business. Professor Rosenthal's careful investigation of the backgrounds of the seventy-nine men elevated to bishoprics between 1399 and 1485 reveals this belief to be only partly true. Less than a quarter were from noble families, a higher figure than a couple of centuries earlier but nothing like a monopoly. More sur-

prising is the author's finding that a third of the churchmen in question had performed no appreciable government service before becoming bishops; though it is true that the most important sees tended to go to experienced civil servants, and that some of the "nonservers" acquired administrative experience in their universities. Most striking of all is the fact that two-thirds of the bishops had previously held archdeaconries. This is perhaps the clinching argument for an "apparatus for recruitment and selection of the ruling elite" that the author suggests, though without ever clearly demonstrating.

Two biographical tables sum up the research behind this brief but valuable monograph. After reading it one longs for more flesh on the statistical bones, and the title seems a bit misleading: what the work is really about is "the cursus honorum which led to a bishopric" (p. 5). But it is nonetheless useful to have the elements in this cursus laid out so thoroughly and compactly.

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JO ANN MCNAMARA. *Gilles Aycelin: The Servant of Two Masters*. [Syracuse:] Syracuse University Press. 1973. Pp. ix, 220. \$8.50.

Gilles Aycelin was, as Jo Ann McNamara puts it, "lawyer, councillor to Philip the Fair and two of his sons, president of the Parlement of Paris, diplomat, Archbishop of Narbonne and of Rouen, presiding officer of the papal commission to investigate the Templars, . . . [and] one of the most influential men of his age." One should welcome, therefore, a study of his career, one that has the potential of illuminating many of the still unresolved issues surrounding the personalities and policies of Philip, Boniface VIII, and their successors.

In a sense, Professor McNamara's work stands in a great tradition. At least since the Second World War medievalists have increasingly turned to detailed biographical studies as a vehicle for understanding the realities of their period, and these efforts have provided a refreshing counterbalance to the overwhelmingly institutional and abstracted monographs that preceded them. Late Capetian France has proved no exception to this development, and in this regard one thinks immediately of the publications of such scholars as Strayer, Pegues, and Favier, to mention only a few.

One has to report, regretfully, that the present book does not live up to the promise of

the tradition or of Gilles Aycelin's career. Much of the difficulty lies with the nature of the research actually done. Because personal circumstances prevented archival work abroad, Professor McNamara has had to rely on published resources, and one agrees with her judgment that they are in fact sufficient, providing that everything available is used. But that is not the case here. Of the 224 items cited in the bibliography, only nineteen were published after 1950, and with the exception of one article by Strayer, nothing in it originally appeared after 1969.

As a result, Professor McNamara appears almost totally oblivious to some of the most significant work done in the last generation. Further, even the works cited are employed with an indiscretion that is wholly unwarranted. For example, Lot's and Fawtier's *Historie des institutions françaises* is footnoted reverently as the final word on any subject under discussion; nowhere does Professor McNamara show any awareness of Fawtier's own cautionary introduction to the second volume in which he makes clear his own reservations about Lot's outdated conclusions, ones that he decided to publish only as an act of piety. And if Lot and Fawtier are overly relied upon, this monograph is ignorant of the latter's inventory of Philip the Fair's documents, a work that would help to plug many of the evidentiary gaps that Professor McNamara is content to slide over with frequent appeals to such formulas as "there can be little doubt that . . ." or "it is possible to imagine that. . . ."

One consequence of these bibliographic failings is an almost total conceptual confusion. At one moment Philip the Fair emerges more pure and saintly than St. Louis; at the next, however, Boniface VIII is so sympathetically treated that Philip quickly reverts to being the "bad guy" of the traditionalists. To cope with such protagonists Gilles Aycelin must have had even more talents than Professor McNamara is endlessly willing to bestow on him.

Combine these defects with reasoning that is evasive and shaky, and terminology that is sloppy and misleading, and one does not end up with a book whose contribution to scholarship is significant. One can only hope that the Syracuse Press will exercise greater discretion in future.

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CHRISTIANE PIÉRARD. *Les plus anciens comptes de la ville de Mons (1279-1356)*. Volumes 1 and 2. Brussels: Académie Royale de Belgique, Com-

mission Royale d'Histoire. 1971; 1973. Pp. xlvii, 785; 213, 5 plates.

For historians concerned with medieval institutional, economic, social, and demographic history the records in this volume constitute a treasury of information on medieval urban institutions, particularly financial; on trade, industry, and transport; on construction techniques, implements, and skills of workers; on urban planning and expansion; and on the names and occupations of those living in Mons, the principal town of the county of Hainaut. Interesting, too, is that these records span the years between 1279 and 1356, a period that saw a leveling off and contraction of the medieval economy; a series of floods, famines, and epidemics that preceded the Black Death; and the opening phases of the Hundred Years' War.

The records of Mons, edited here for the first time, are among the earliest and best preserved of the urban financial records of the Low Countries. They consist of three categories. Most valuable are the *comptes en rouleaux* of the *massarderie* consisting of an annual accounting by the *massard* (the collector of urban revenues) to the *échevins*. Closely associated with these are other special accounts submitted to the *massard* by subordinate officers responsible for various administrative functions such as public works, payment of *rentes* and pensions, collection of taxes, and public assistance that were often appended to the account of the *massard* and served as *pièces justificatives*. A third category, the accounts of the *échevins*, consists of a statement of the financial situation of Mons and was rendered yearly on June 24 by the *échevins* whose term had expired to their replacements. Until 1338 these accounts were enrolled on pieces of parchment sewn end to end, a technique similar to that used by the English chancery in its enrollment of such records as the charters, letters patent and close, and writs of liberate. In 1338 they began to be placed in registers written on paper, a system continued to 1602. That so many of these records have been preserved is due to the good sense of the responsible officials who made one or two copies of each account.

With these accounts now published historians concerned with urban history can compare the urban administration of Mons with that of the Flemish towns of Ghent, Ypres, and Bruges, whose financial records have been edited by J. Vuylsteke, G. Des Marez, E. De Sagher, C. Wyffels, and J. De Smet. Besides yielding details on urban finance and economic activ-

ities, these accounts also illuminate the relations of Mons with the counts of Hainaut. By the late thirteenth century rarely did a comital official interfere with the internal government of Mons, which was under the control of an independent *échevinage*. This urban independence continued until Hainaut came under the control of Philip the Good of Burgundy, who then reimposed central control over the towns and forced the *échevins* to render annually a financial accounting to ducal officials.

Written in the Franco-Picard language and tinted with a large number of local words, these accounts should also be of interest to the philologist. Study of the vocabulary of Montois laborers of the nineteenth and twentieth centuries shows it similar to that of their ancestors in the thirteenth and fourteenth centuries. A glossary in volume 2 of words used most frequently lists, for example, the word *acheter* appearing as *acater*, *accater*, or *akater*, and the word *cing* as *chiunch*, *chiunc*, *ciunch*, *ciung*, or *chienc*.

This is an excellent edition and, as one has come to expect, is superbly printed by the Commission Royale d'Histoire.

BRYCE LYON

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HENRI HUGONNARD-ROCHE. *L'œuvre astronomique de Thémon Juif, maître parisien du XIV^e siècle*. (Centre de Recherches d'Histoire et de Philologie de la IV^e Section de l'École pratique des Hautes Études. Fifth Series, Hautes études médiévales et modernes, 16.) Geneva: Librairie Droz. 1973. Pp. 429.

This book is a study of the life and works of Themo Judei, a very learned Parisian master of the fourteenth century, and it is accompanied by a detailed analysis of Themo's *Questions on the Sphere of Sacrobosco* and *Question on the Motion of the Moon*. The book also includes a critical edition of the latter, established from the three extant fourteenth-century manuscripts. This study, undertaken under the guidance of Guy Beaujouan, earned its author the title of "Élève diplômé de la Section des sciences historiques et philologiques de l'École Pratique des Hautes Études." It exhibits the same merits and drawbacks as a good American Ph.D. dissertation in the history of medieval science. It is scholarly, competent, and learned, and it shows that Mr. Hugonnard-Roche has mastered the techniques of study of medieval manuscripts and has otherwise learned the historian's trade. It also contains little mistakes

of, first, interpretation, second, of information, and third, and the most damaging of all, of misplacing numerous anachronisms and anachronistic transcriptions of medieval rhetorical discussions of mathematical topics into the modern language of symbolic algebra. Examples of mistakes of interpretation are that Aristotle's views on vision seem to be reduced to an intromission visual rays theory (p. 26) and that Themo's method is repeatedly and unwarrantedly called "experimental" (pp. 36, 48, 53, and elsewhere), while, strangely enough, on page 195 it is stated that "la conception de la causalité exposée par Thémon conduit donc à refuser toute valeur à l'observation ou à l'expérience et apparaît incompatible avec la formation d'une science expérimentale!" Examples of the second kind of mistake made are, first, that the hypothesis that all medieval references to Apollonius's *Conics* come from the Arabic translation is accepted uncritically (p. 32) and, second, that *De crepusculis et nubium ascensionibus* is mistakenly ascribed to Alhazen (p. 36).

As to the third area of mistakes made, Henri Hugonnard-Roche seems much too eager and overly zealous to translate ancient and medieval descriptions into symbolic language without further ado. Furthermore, he does not seem fully aware of the potential historical dangers involved when this ahistorical procedure is used indiscriminately. Thus, to mention only two examples (though instances literally abound), in speaking of Themo's proof of the "Merton Rule," which follows exactly William Heytesbury's proof in the *Probationes conclusionum tractatus solvendi sophismata*, Mr. Hugonnard-Roche says that "ce raisonnement est essentiellement une opération de calcul infinitésimal qui pourrait s'écrire: . . . $dS_e/dt - d(S_e/2)/dt = d(S_e/2)/dt - dS_b/dt$. . ." (p. 233), and that "ce raisonnement tout intuitif est, ainsi que le note M. Clagett, une opération de calcul infinitésimal" (p. 235). The reasoning in question is, most obviously, not a calculus operation for Themo, but it is indeed an intuitive operation, which is not the same thing!

There are also anachronisms in interpreting medieval concepts. Again a couple of examples will suffice: where Themo speaks of magnitude, the author interprets this as meaning mass (p. 88); and, on page 177, the scholastic distinction between *virtus fatigabilis* and *virtus infatigabilis* foreshadows for the author the concept of energy!

The two treatises analyzed by Mr. Hugonnard-Roche, though outwardly astronomical in character, are the product of a very widely read

author who was not an astronomer, but a natural philosopher. They do not deal, therefore, with the main problem of astronomy, namely, saving the celestial phenomena as accurately as possible by means of an appropriate geometrical model. Themo accepted without any critical discussion Ptolemy's theory as the best geometrical representation of celestial appearances, and he tried to establish in his *Questions on the Sphere of Sacrobosco* the points of agreement, as he saw them, between the Ptolemaic hypothesis and Aristotelian physics. In this investigation Themo's sympathies lie squarely with the Stagirite.

Themo's positions, furthermore, are very close to those of the famous late scholastics. His dynamical ideas are indeed very similar to those of Albert of Saxony and Nicole Oresme. The doctrines expounded in *Questions on the Sphere* are quite often identical to Pierre d'Ailly's ideas in his own *Questiones in sphaeram Johannis de Sacrobosco*, in which d'Ailly might have used Themo's work. It is, however, impossible to establish accurately Themo's place among his contemporaries before reliable, scholarly editions of Oresme's *Juvenilia* are published.

The *Question on the Motion of the Moon* is a philosophical quodlibetal question, patterned after the older theological quodlibetal questions. Indeed, like the latter, it contains both a *disputatio* and a *determinatio*. Dealing, atypically, with very recent sources (the works of the Mertonians, written between 1328 and 1350), it represents a transcription of a summary made by Themo in the wake of a public disputation that took place at Erfurt in 1350, and it is an interesting testimony on the teaching and organization of the sciences in the middle of the fourteenth century.

The issues discussed in it have to do with a basic cosmological topic in Aristotle, namely the intrinsic analysis of local motion as it applies to celestial motions. The question specifically asked is, does the moon move uniformly or not, and if not, how? *Uniformiter difformiter* or *difformiter difformiter*? Themo transfers Bradwardine's version of Aristotle's dynamical law to the kinematical domain. Furthermore, his ways of treating intensities of qualities, borrowed from the Mertonians, enable him to perform some remarkable mathematical feats: the summation of an infinite number of parts of a continuous magnitude by means of the summation of a decreasing geometrical progression and the enunciation and proof of a number of kinematical propositions that, when represented graphically (which Themo does not

do), lead most naturally to Oresme's famous diagrams.

It is clear that Themo's methods represent a softening and mellowing of Aristotelian categories, and they prepare the way for considering velocity as an entity in its own right. Also, speaking explicitly in terms of spaces and times rather than forces and resistances (a procedure made necessary by the fact that there are no forces and resistances in the heavens) can be seen as a necessary step toward the unification of celestial and terrestrial motions under the same law. With Oresme, Themo is one of the first on the Continent to employ the results and methods of the Mertonians.

SABETAI UNGURU

University of Oklahoma

JEANNE LAURENT. *Un monde rural en Bretagne au XV^e siècle: La quévaise*. (École Pratique des Hautes Études—VI^e Section. Centre de Recherches Historiques. Les hommes et la terre, 14.) Paris: S.E.V.P.E.N. 1972. Pp. 440. 82 fr.

Expanding her *thèse* for the École des Chartes, Mlle. Laurent presents a valuable and comprehensive study of the *quévaise* land tenure system found in Bretagne as well as a description of the rural world in which it existed. She also publishes the texts of the documents used to define the *quévaise* and to discover why it developed and how it operated. The most valuable aspect of this book, however, is its demonstration of methodology. With great skill and creativity Laurent shows how much information can be gleaned from relatively few documents that are fully exploited. From the existent material she constructs a picture of rural life and shows how the law governing the *quévaise* worked itself out in practice from the fifteenth through the eighteenth century. She painstakingly supports each of her points with relevant examples from the documents and indicates how her conclusions differ from those of other historians.

The *quévaise* itself was a form of land tenure found primarily in the holdings of the Cistercian and Hospitaler orders located in Basse Bretagne. Laurent forcefully demonstrates that the holder of a *quévaise* retained considerable freedom vis-à-vis the other tenants in the region. He could be deprived of the *quévaise* only if he was absent for a year and a day, and the obligations to his ecclesiastical lord were relatively light and easily commuted into money payments. Laurent claims that the *quévaise* developed in the thirteenth century to encourage

men to settle on monastic holdings in order to clear new lands for cultivation. The settlers were given a plot of ground and a house free of taxation as a base from which to work, and they could pass the holding on to their heirs according to the right of *juveigneurie*. Laurent maintains that the *juveigneurie* functioned well in the *quévaise* because the frontier situation allowed the older sons to acquire their own *quévaises* nearby. She also shows that while *quévaises* existed to the Revolution, from the sixteenth century many *quévaisiers* attempted to use their unique freedom of tenure to identify themselves as part of the feudal structure.

Laurent's book is particularly valuable as a source of information about research methodology to graduate students preparing to work in rural history. The book will also interest historians seeking additional insight into the development of Breton rural institutions and those individuals who enjoy seeing another scholar do her job very well.

ROBERT S. TRULLINGER, JR.

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FRANK D. PRAGER and GUSTINA SCAGLIA. *Mariano Taccola and His Book De Ingeneis*. Cambridge, Mass.: MIT Press. 1972. Pp. xii, 230. \$17.50.

Mariano di Jacopo, surnamed "Il Taccola" (ca. 1382–ca. 1455), of Siena was an artist and notary who was famous in his day as now for his drawings and descriptions of technical devices. From the many Taccola manuscripts Frank Prager and Gustina Scaglia have reconstructed two treatises, *De Machinis* 1449 (1971) and *De Ingeneis*, completed in 1433. Mariano's works are especially significant as sources for the history of Italian technology—and art—in the century that culminated with Leonardo da Vinci. Prager and Scaglia have collaborated before to illuminate portions of this landscape. Their study of Brunelleschi (1970) showed a felicitous combination of the skills of the historian of technology (Prager) and of art (Scaglia). Scaglia independently published *De Machinis* in 1971. Here the authors treat Taccola's life and work, his position as an artist, the roots of *De Ingeneis* and the work's relation to *De Machinis* and to later copybooks. Their reconstruction of the autograph text, which is now divided between Florence and Munich, is a masterful piece of careful historical craftsmanship.

Yet, to this reader, their treatment of the treatise represents a serious disappointment. Although their scholarship is up to its usual

high standards, *Taccola* is a work gravely flawed by its format. The book as a technical product undermines the quality of its contents. Some questions by way of illustration: Was *Taccola* originally intended as an edition of *De Ingeniis*? If so, why is approximately half of the original manuscript omitted? If it was intended as a study, why is Mariano's original (and sometimes jumbled) order of drawings preserved? Why do the authors repeatedly refer to drawings by Taccola that are not published here or elsewhere? Why are folios from the original reproduced in Lilliputian format, making details of the drawings indistinct and lines (indeed, whole pages) of text illegible? What possible economies are effected by such measures when pages with figures are left half blank? In short, what went wrong with this work between conception and publication?

We would seem to have here an example of a publishing dilemma. Given that facsimile editions of source documents are needed badly in many fields of historical scholarship and that sharply rising publication costs threaten to price such volumes out of their intended market (at \$100 and up per volume!), then we should ask what sort of compromises should be made in order to make important source documents available. Perhaps MIT Press is to be commended for its concern with this problem, but if *Taccola* is any example, surely the cure is worse than the disease. Compromises that vitiate the scholarly purpose of a work, and threaten to drive serious readers to madness, through ill-conceived and awkward formats are unacceptable. Some other solution will have to be found.

BERT S. HALL

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MODERN EUROPE

JOSÉ ANTONIO MARAVALL. *Estado moderno y mentalidad social (Siglos XV a XVII)*. In two volumes. Madrid: Ediciones de la *Revista de Occidente*. 1972. Pp. xiii, 529; 619.

José Antonio Maravall is a professor of political science at the University of Madrid and Spain's leading student of political ideas and theories for the late medieval and early modern periods, primarily from the fifteenth through the seventeenth centuries. The two volumes under review constitute a massive synthesis and compendium of the new ideas concerning policy and state structure in Western Europe during that period, together with the new concepts that emerged

concerning social roles and organization. The work is devoted exclusively to theories, ideas, and presuppositions; it is not a study of empirical history that analyzes the course of events or the chronological development of certain institutions in specific states. The field of study includes all of Western Europe, but the primary focus is Spain, particularly in volume 2, which deals with more specific ideas of government organization and functioning.

Perhaps the most interesting and original part of the entire work is the second half of volume 1, "Power, Individual, Community." In this section Maravall analyzes the new concepts of state power, sovereignty, and the claim to "absolutism." He agrees with many other students that the West European "absolute monarchy" was not really absolute in the sense of total institutional power, but he defends the continued use of the theory since this terminology was employed in the sixteenth and seventeenth centuries and corresponded to the pre-eminent political sovereignty of the new monarchist state. Maravall discusses the pre-constitutional framework of "fundamental laws" that was accepted by all Western states at that time, while emphasizing the vagueness of such a corpus. Other chapters deal with the parallel emphasis on property rights, individual liberty, and the sense of nation and community that accompanied development of the early modern state.

There is not very much in these two volumes that may be considered novel or specifically original. The discussion of economic attitudes and policies in volume 2 is merely a rehash of what is already known by students of Spanish economic history in this period. However, the erudition in secondary works, literary sources, and contemporary works of law and political theory is immense. Taken simply as a study in ideas, Maravall's new work may serve as a useful compendium of new social and political concepts during the period of transition from the late Middle Ages. The author's assertion that "this book probably presents a new way of seeing the history of Spain" (vol. 1, p. 5) is considerably exaggerated, but his product will be a useful reference for the new trends in fifteenth- and sixteenth-century theory.

STANLEY G. PAYNE

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SERGIO BERTELLI. *Ribelli, libertini e ortodossi nella storiografia barocca*. (Biblioteca di storia,

6.) [Florence:] La Nuova Italia. 1973. Pp. xviii, 377. L. 3,600.

In this lively and interesting volume Bertelli, a distinguished scholar in the field of Renaissance and post-Renaissance studies, concerns himself with the evolution of historiography from about the third decade of the sixteenth century until approximately the end of the seventeenth century. He calls this period baroque and defends this use of the term in the foreword.

In a world founded on religious faith and organization, with attendant superstition and authoritarianism, the development of a methodology on the part of writers on whom we would confer the title of historians was slow and laborious by necessity. It is this process of transformation that Bertelli considers in his study with copious documentation. He believes that the history of this historiography can be written only after well-documented observations are studied and discussed by specialists. He hopes that his work will be thought-provoking and will start fruitful discussions.

Bertelli's point of departure is the anti-Renaissance, that is, the reaction to the Renaissance, which created a new set of values, dictated by both the Protestant and Catholic revolutions. Machiavelli, and also Guicciardini, were rejected due to their rationalism and their lack of religiosity, and because their world had abruptly ended in 1530 with the defeat of the Republic of Florence and the affirmation of the supremacy of Spain in Italy. Their contribution was to become vital again a century or more later. For the moment the principle of authority prevailed in all aspects of life and it became inevitable that the tendency to codify and dogmatize everything should also affect historiography.

Bertelli's work is divided into two parts, each covering in general the same period of time but from different perspectives. Indeed, this method is followed also in the chapters into which each part is divided.

In the first part our author concentrates on religious history, in the second on the history of different peoples, nationalities. He shows how certain lines of force emerge and develop from clashes between groups attempting to defend, support, or justify opposing points of view and power supremacy. The clash between Protestant and Catholic historical writings, at the beginning largely based on assertions, later leads to the rediscovery of the importance of documents in supporting the arguments pre-

sented. Still later, recognizing but unwilling to admit the rightness of certain affirmations of the opposition, the clash loses aggressiveness and leads to research of an erudite kind. A similar cycle occurs in the historiography that the French, English, and other peoples develop in order to demonstrate their nation's complete independence from foreign influences, particularly from ancient Rome and Renaissance Italy.

Rebels, orthodox, libertines are terms used by the author to indicate succinctly the interplay of the various forces from which modern historiography will develop.

GRAZIA AVITABILE
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JAMES M. STAYER. *Anabaptists and the Sword*. Lawrence, Kansas: Coronado Press. 1972. Pp. ix, 375. \$10.00.

Having published a number of articles in the last ten years describing the positions taken by various Anabaptist sects on violence, authority, and church-state relations, Professor Stayer has now put together his conclusions in this book. The main problem in writing such a book is how to reconcile, within the confines of a single movement, such wildly divergent standpoints as the chiliastic zealotry of a Thomas Müntzer, the *Realpolitik* of a Balthasar Hubmaier, and the apoliticism of a Menno Simons. The difficulty is compounded by the vastly differing milieux in which Anabaptism flourished: Switzerland, southern Germany, Bohemia, the Netherlands, almost the entirety of the possible political spectrum.

Stayer attacks the problem in the only reasonable way, by treating the various Anabaptist movements separately, without attempting to impose an artificial doctrinal or organizational unity upon them. He also keeps scrupulously to the middle ground between those who, basing themselves chiefly on the testimony of the Anabaptists' enemies, depict them as dangerous fanatics, and the writers of partisan martyrologies. Making use of a wide variety of sources, Stayer has produced what will surely be for some time to come the definitive treatment of all shadings of Anabaptist positions on worldly authority. It can be said, however, in criticism of his achievement that he has defined his task too narrowly. One finds almost everything about the subtlest and most arcane differences between the opinions of the various sectarians, not nearly enough about the contexts of these differences. One has the feeling, at times, that

one is reading a taxonomy of doctrine, whose compiler is somewhat indifferent to both the generalized socioeconomic upheaval of the sixteenth century and the overall irenic tradition. There are curious omissions from the bibliography: Peter James Klassen's *The Economics of Anabaptism, 1525-1560* and Leonhard von Mural's *Glaube und Lehre der schweizerischen Wiedertäufer*, to cite but two, both works that might have added depth to the treatment. In sum, Stayer's book falls rather short of its goal.

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HARVEY MITCHELL and PETER N. STEARNS. *Workers & Protest: The European Labor Movement, the Working Classes and the Origins of Social Democracy, 1890-1914*. Itasca, Ill.: F. E. Peacock Publishers. 1971. Pp. v, 250. Cloth \$6.00, paper \$3.95.

This slender volume is an interesting publishing experiment. Harvey Mitchell and Peter Stearns have each contributed less than book-length essays taking divergent approaches to the study of the West European labor movement in the decades before World War I and then responded briefly to each other's analyses. Their debate is prefaced by Robert Wohl's critical introduction. Most of the key problems in late nineteenth-century labor history are raised in the course of these essays and this fact, along with the historiographical conflicts they pursue, will make this book a popular choice for classroom use.

Mitchell's essay alone would justify adoption. In one hundred pages he presents an excellent outline of the principal trends in the ideological and organizational development of unionism and working-class politics in Great Britain, France, and Germany from 1890 to 1914. Of necessity there is a certain amount of oversimplification (especially in his picture of the global economic development in each nation), but I know of no summary in English of the formal labor movement that captures the story with such clarity and brevity. There is no question, however, that Stearns is quite correct in terming this "conventional" labor history. The subject matter with which Mitchell deals—the growth of national political and union organization, the pronouncements of leaders, the resolutions of national congresses, the rhythm and nature of major strike activity—is well worn and time honored. His general theses, (1) that differences in national modernization pro-

cesses make it virtually impossible to analyze the European labor movement as a unified whole during this period and (2) that the apparent trend toward reformism everywhere in Western Europe after 1900 (England possibly excepted) was rooted in the timidity of leadership and the bureaucratization of structures, are again unoriginal.

Stearns, on the other hand, is seeking to develop a long-needed new perspective on the labor history of the period. He wants to penetrate the world of working people themselves and to reconstruct labor history "from the bottom up." He also feels that with such an approach, one may discover more easily comparable trends in the entire West European labor movement and therefore begin to move away from national histories toward a more integrated view of labor's experience as a whole. In short, he is aiming at a broader social history of the working classes and an understanding of its implications for the formal labor movement. All of this deserves unqualified praise. But Stearns goes well beyond this methodological challenge. Freely admitting that the study of working-class social history is in its infancy, he nevertheless proceeds to offer this core interpretation: that a majority of workers in this era were social and political moderates (and became more so with the passage of time), and it was this fact, not the opportunism, irresolution, or bureaucratic mentality of their leadership, that pushed the movement toward reformism and even toward an accommodation of industrial capitalism. Such an interpretation was, of course, not uncommon at the time. Most capitalists wanted to believe it, and Lenin's opinion that the workers in advanced capitalist countries would only develop a "trade union consciousness" became one of the key ingredients in his theory and strategy of revolution in Russia. The question of what workers really wanted in this crucial age is undeniably one of the most important a historian of the modern world can ask. Unfortunately Stearns falls far short of proving his thesis. My immediate expectation was that he would produce a host of examples drawn from local studies, histories of individual unions and their locals, contemporary local newspaper accounts, local party and union meeting records, trade journals, court records, the fairly numerous worker autobiographies, diaries, and letters that date from this period. But little evidence of this sort seems to have been used. In all fairness, it is possible that the brevity demanded by the format of this book caused him to delete

much material of this order. His case is left to rest upon evidence already generally familiar to us. He comes closest to developing a convincing argument in discussing strike goals, which indeed were generally specific, short range, and unideological. Still, Mitchell points to political strikes in various countries and both agree that many British strikes in the years just before the war can be viewed as quasi-revolutionary. But there remains a more general problem. What is, after all, the function of a strike? Its very nature and the normal circumstances under which it occurs all but assure that it will be a pragmatic instrument for achieving immediate goals. Overall, then, Stearns's stirring call for approaching labor history from below remains an important contribution. But to draw general interpretations—especially one of such significance—from the skimpy evidence currently available is surely premature.

It must be said, finally, that both Stearns and Mitchell are hardly dogmatic in maintaining their respective positions. The dialogue is clearly open-ended, and we can look forward to many more years of research and debate on the issues raised in this book. There is one area, however, where both seem to think that the debate is closed—that the West European labor movement became less militant as 1914 approached. Wohl is concerned that such a thesis, if carried too far, will seriously distort our understanding of the radicalization that accompanied and followed World War I and the Russian Revolution because these external events are surely not the only causes of the later process. I can only echo this concern and hope that the intensive study of labor history at the grass-roots level and of the actual relationship between workers and their leaders will shed the needed light on this and the other basic questions posed by these essays.

CHRISTOPHER H. JOHNSON
Wayne State University

A. A. GUBER *et al.*, editors. *Rossii i Italiia: Materialy IV Konferentsii sovetskikh i ital'ianskikh istorikov, Rim 1969. Russkii i ital'ianskii sredne-vekovyi gorod; Russko-ital'ianskie otnosheniia v 1900–1914 gg.* [Russia and Italy: Material from the 4th Conference of Soviet and Italian Historians, Rome 1969. The Russian and Italian Medieval City; Russian-Italian Relations, 1900–1914]. (Akademiia Nauk SSSR, Institut Vseobshchei Istorii, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 476.

This volume contains the proceedings of the Fourth Conference of Soviet and Italian His-

torians held in Rome on October 25–26, 1969, which considered "mediaeval" Russian and Italian cities and Russo-Italian relations, 1900–14. The book contains three papers on each topic with a variety of commentaries ranging from critical evaluations through short research essays to occasional remarks. The session on cities was the better of the two, offering a comparative typology for Italian urbanization, a study of the conflict between the nascent bourgeoisie in sixteenth-century Russian towns and political centralization based on feudal relationships, and an analysis of governing structures in medieval Novgorod. Overt ideological questions played almost no part, though the common philosophical base for Soviet historical research showed clearly in the papers and comments, as did the range and depth of Soviet scholarship on non-Russian subjects. The twentieth-century session was less satisfactory. Assiduous readers will find nuggets of fact concerning diplomacy, trade, or Italian public opinion, but the main problems discussed were Lenin's "fatalism," varying perceptions on democratic revolutions, and data supporting Lenin's assessment of capitalist development, imperialist competition, and the dynamics of international relations. On balance, the conference was probably most valuable to the people who participated in it. The papers add little more than volume to what is already available in the published literature, though Professor Ernesto Sestan's typology re-emphasizes the importance of comparative analyses, and N. E. Nesov's essay on sixteenth-century Russia would be useful to scholars, who are not familiar with the Soviet literature, studying early European state-building processes. But this raises a broader issue. The tactical advantages of an accepted historical schema that asks the same questions about different historical cultures, thereby generating masses of mutually reinforcing data, become dramatically obvious from the Soviet contributions to this book. Similarly, the potential for distortion, always an important factor in such monolithic systems, is clear, particularly in the Soviet essays on the twentieth century. This fact should not, however, be considered grounds for dismissing Soviet primary scholarship out of hand. Non-Soviet Russian historians, though often critical of Soviet method and historical philosophy, have gained much from Soviet research, and now there is an established and growing corpus of Soviet work on non-Russian subjects that deserves to be more widely known. Given the comparative context for the conference, it was certainly pure cultural chauvinism

to publish this volume in Russian rather than one of the more widely accessible European languages, but this only underlines the need for non-Slavic specialists to consider adding Russian to their arsenal of scholarly tools, while routinely reviewing Soviet contributions to their respective fields. To do otherwise is to ignore a burgeoning historical literature, or worse, to reject it without critical examination.

RODERICK E. MCGREW
Temple University

KARL J. NEWMAN. *European Democracy between the Wars*. Translated by KENNETH MORGAN. [Notre Dame, Ind.:] University of Notre Dame Press. 1971. Pp. 475. \$12.00.

A number of interesting and important questions are raised in this book: the central fallacy of the myth on which democracy is based, the fundamental conflict between equality and liberty; the possibility of its functioning in any but a small community; and the impact of the growth of numbers and of an increasingly complex technology on the organization of the state. Within this framework, the failure of the democratic experiment in the Central European milieu, the German most of all, constitutes the focus of the analysis.

The rash of post-1918 democratic constitutions, most of them modeled on the French, held in it the seeds of failure, for these constructions were suddenly imposed upon societies in which they lacked roots. There was also the problem of ethnic minorities in most of the new states; self-determination, the foundation on which their existence rested, was in large measure denied to the newly created minorities.

In these circumstances authoritarian rule in varying degrees was the alternative solution. Italian fascism was the first model, but far more threatening was German nazism to which the impact of economic crisis gave the opportunity. This new development gave rise to two questions. One was the threat inherent in renewed German power; the other derived from the fact that nazism as a philosophy, a view of the organization of the state, was applicable to milieux other than the German. But here in turn a contradiction was created by the narrowly nationalistic and racial content of nazism. The effect was confusion. To turn German minorities in other states into fifth columns—the Sudeten case is the outstanding example—was relatively easy; but where could inferior Slavs turn who might agree with other aspects of the Nazi creed?

These are the questions discussed in perhaps unnecessarily repetitious and lengthy detail. If the analysis is sound and contains valuable observations, some caution seems warranted in the use of this book. Some of the interpretations are questionable, not to mention careless editing, typographical and other errors. To present the Ruhr episode of 1923 as almost the result of a conspiracy between Poincaré, Mussolini, and Stinnes calls for stronger evidence. And the version presented of Beneš's role in conveying information to Stalin is, to say the least, unorthodox. Certainly, it is not what one finds in Churchill's wartime memoirs, cited as evidence, incidentally with an incorrect reference. In sum, a useful book but also a mixed bag.

RENÉ ALBRECHT-CARRIÉ
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C. J. C. MOLONY *et al.* *The Mediterranean and Middle East*. Volume 5, *The Campaign in Sicily, 1943, and the Campaign in Italy, 3rd September 1943 to 31st March 1944*. (History of the Second World War, United Kingdom Military Series.) London: H. M. Stationery Office; distrib. by Pendragon House, Palo Alto, Calif. 1973. Pp. xix, 921. \$42.00.

No theater of operations in World War II was the subject of so much debate and, in the post-war years, of as much controversy as the Mediterranean. In the context of the cold war and of Soviet expansion, it seemed that Allied wartime strategy in the Mediterranean should have been directed, as Churchill supposedly urged against strong American opposition, toward the Balkans, the "soft underbelly" of Europe. This view is largely fiction. The British never seriously proposed such a strategy; the differences between them and the Americans were not over basic strategy but over priorities and timing. On the necessity for an invasion of northwest Europe there was no disagreement.

This volume, though it touches on these questions, deals primarily with operations. Fifth in the six-volume official British history of the war in the Mediterranean and Middle East, it opens with the invasion of Sicily in July 1943, the first large Allied landing on the Continent, and then describes in six chapters the subsequent surrender of Italy and the hard-fought thirty-eight-day battle to secure that mountainous island. The invasion of Italy by way of the Straits of Messina and the landing at Salerno early in September raised the curtain on the long and arduous campaign up the Italian boot to the Winter line and the great Ger-

man fortress at Cassino. There the Germans held fast, foiling the Allied amphibious encirclement at Anzio, until March 1944 when Cassino finally fell before repeated Allied assaults. The drive to Rome, the move north through the Gothic line to the Po Valley, and the surrender of German forces in Italy in May 1945 are to be covered in the concluding volume of the series.

The decision to go to Sicily was made at Casablanca in January 1943, at the height of the struggle for North Africa. It was reluctantly agreed to by the American planners who not only feared that this move would divert forces from the main effort in northwestern Europe but also suspected British motives in pressing for further action in the Mediterranean. But there was no help for it. The invasion of France was at least a year away and the troops that had driven the Germans and Italians from North Africa could hardly be allowed to remain idle. There were other reasons for moving ahead in the Mediterranean, not the least of which was the advantage to be gained by opening the area to Allied shipping. Subsequent meetings of the Combined Chiefs in Washington and Quebec during May and August of the same year confirmed this decision and authorized such other operations as would contain the largest number of German divisions and take Italy out of the war. Such operations, it was understood, were to be subordinate to and supportive of the main effort in France. It was with this understanding that the invasion of Sicily was carried out and the campaign in Italy undertaken.

Fortunately Hitler proved most cooperative. He, like Churchill, had his eye on the Balkans, and on October 4, whether because of his fears of an Allied invasion as this volume suggests or for other reasons, he ordered Kesselring to stand firm instead of withdrawing slowly, as he had been doing. He was to hold the Allies in place, Hitler told him, and in the event the Allies showed any sign of crossing to the Dalmatian coast, he was to attack forthwith. Up to now small German forces had been giving ground slowly, skillfully utilizing the difficult mountainous terrain to tie down Allied forces. The effect of this new order was to commit a larger number of German divisions to the Italian front, troops that could have been used in Normandy, thereby ensuring the success of Allied strategy.

This is a large volume, almost one thousand pages with maps, plates, and tables to match. One would hardly expect that so large a volume

on so thin a slice of the war, one so burdened with the imprimatur of official history and an impossible title (really a nontitle), and written by a team of military men from different services could hold the attention of any but the most dedicated specialist. Yet it does so. It is lucidly written, in simple and at times moving prose, remarkably free of jargon, judicious in tone (as in the treatment of General Montgomery), and well paced. The authors have a keen eye for terrain, the ability to explain intricate matters clearly, and are equally at home with the decisions of the Combined Chiefs and the actions of small units along the Winter line or amphibion operations off the coast of Sicily or Anzio.

Unlike the Americans, the British tell the story of the military side of the war from the defense rather than the service level. The result is a comprehensive and total view of the war that is largely lacking in the separate U.S. Army, Navy, and Air Force series. In one respect, however, the American volumes are superior: most of them are fully documented and include extensive bibliographies. The British decision to omit documentation, understandable perhaps at an earlier period, is hardly defensible now, thirty years after the war. But even with this limitation, this volume with its predecessors is likely to remain the standard and definitive account of the British side of the struggle in the Mediterranean, a monument to the industry, devotion, and skill of its authors.

LOUIS MORTON
Dartmouth College

RALPH MERRIFIELD. *Roman London*. New York: Frederick A. Praeger. 1969. Pp. 212. \$9.50.

TIMOTHY BAKER. *Medieval London*. New York: Praeger Publishers. 1970. Pp. 260. \$11.95.

MARTIN HOLMES. *Elizabethan London*. New York: Frederick A. Praeger. 1969. Pp. 123. \$5.95.

The deplorable lack until recently of good general histories of London is being dealt with by several enterprising publishers bringing out whole series of period histories of the metropolis. The volumes under review place particular emphasis on London's physical structure and appearance and especially on those material remains that can still be seen. Each could serve as an admirable guidebook for the historically minded tourist determined to discover what underlies the present city.

Determination is necessary, for little of the London of the millennium and a half that

preceded the Great Fire is any longer there. London's very success has obliterated its past. Even had there been no fire, it is doubtful that we would have much more left of medieval and Tudor London than we do; while land values and the zeal of building developers being what they are, London by 1974 would probably look much as it does had not a single bomb fallen on it in 1940. The skills of the archeologist and the knowledge of the museum curator need to be combined with the imagination of the historian to re-create the vanished city. The three authors, respectively assistant director of the Guildhall Museum, editor of the *Victoria County History of Middlesex*, and an authority on Elizabethan costume and armor, combine the specialist's obsession with detail and the historian's search for broader relevance.

In Collingwood's terms they raise what might have been mere chronicle to the level of history by discerning the thought contained within the material shell. In their careful descriptions of buried foundations, fragments of pottery, coins, household implements, artisans' tools, street patterns, and monumental inscriptions they never lose sight of the question: what does it all mean? What, that is to say, can the scattered remnants of London's past tell us about the life lived in its buildings and the values that informed their builders? Their degree of success in finding satisfying answers serves as a challenge to the modern urban historian to make better use of his eyes: if so much can be learned about the life and nature of Roman, medieval, and Tudor London from cooking pots and tessellated pavements, from the foundations of vanished roads and artifacts dredged from the bottom of the Thames, how much more can we learn about Georgian and Victorian London, large chunks of which stand staring us in the face, not as antiquities arranged in display cases, but as integral working parts of today's metropolis!

Of the three, Merrifield's volume on the Roman city has the most to offer the specialized scholar. Unlike the other two it is provided with footnotes and serves as a useful guide to the recent archeological literature on the many discoveries being made in the city. Baker and Holmes provide less that is new, but, given the more abundant written evidence for their periods, they are able to give fuller descriptions of London as a functioning entity. Baker makes excellent use of William Fitz Stephen's vivid if adulatory description of the London of Thomas Becket, while Holmes finds in Elizabethan dramatic literature numerous passages

that illuminate the urban experience. The many illustrations contribute a great deal to an understanding of the text of all three.

In his recent inaugural lecture at Leicester University, Professor H. J. Dyos argued that "the authentic measure of urban history is the degree to which it is concerned directly and generically with cities themselves and not with the historical events and tendencies that have been purely incidental to them." Merrifield, Baker, and Holmes meet his criterion, and if their treatments pay less attention to London as a part of the larger European economy or as a laboratory for the examination of demographic, social, and economic change than many urban historians today would, their London is never merely the stage on which the play is performed, but the play itself. They treat, again in Dyos's words, "the city as such, the whole interlocking apparatus without which urban life could not function, the relations between generations of buildings and generations of men." And while they do not neglect, quoting Baker, "the dirt, the stench, the noise, the hectic indulgence, the violence and the suffering," they also account for the fierce devotion that London, then as now, inspired.

The three volumes represent the best sort of high popularization, and they combine the virtues of the old urban history that moved from monument to monument, inscription to inscription, literary association to literary association with the more recent concern with the city as central to an understanding of the total human experience.

DONALD J. OLSEN
Vassar College

PAUL CERNOVODEANU. *England's Trade Policy in the Levant and Her Exchange of Goods with the Romanian Countries under the Later Stuarts (1660-1714)*. Translated by MARY LĂZĂRESCU. (Bibliotheca Historica Romaniae, Economic History Section. Studies, 41[2].) Bucharest: Publishing House of the Academy of the Socialist Republic of Romania. 1972. Pp. 156. Lei 8.25.

Paul Cernovodeanu presents a thorough picture of late seventeenth-century English trade with the Black Sea areas that presently constitute Romania. His study is based to a large extent on Savary's *Dictionnaire universel de commerce* (1726) and modern published works—to which his notes form a valuable guide. Cernovodeanu did some work at the British Museum and Pub-

lic Record Office in London and evidently exhausted the limited manuscript sources in the state archives of Bucharest, Braşov, Sibiu, and Cluj. The latter relate almost exclusively to trading conditions in Transylvania.

After an initial Marxian gloss on the nature of British trade with the northeastern Balkans in the period, Cernovodeanu rightly concludes that the English policy of trade expansion, while consistent with England's general ends, was largely a function of the needs of the Levant Company. The latter sought trade connections in the Black Sea area because its transit trade in oriental spices had been pre-empted by the East India Company. Cernovodeanu was evidently unable to search the Chancery Masters' Exhibits in the Public Record Office; had he done so the merchant records there would have borne out his argument.

Despite their desire the Levant merchants were never able to make direct contact with the Black Sea lands because of the restrictionist policy of the Porte. Instead, Greek Orthodox merchants of various Balkan ethnic groups did penetrate the area and then resold the goods to English merchants at Constantinople. This penetration by merchants from the south was facilitated by the difficulties that Eastland and other British and West European merchants encountered in their established trades with the area, principally in potash, because of the political disintegration of Poland.

After reading Cernovodeanu's copiously documented argument one is left wondering why the study was undertaken, at least with its English emphasis. Despite the fascinating picture of internal trade developments that he presents, direct English trade was virtually nonexistent by the author's own account and was about to undergo a complete eclipse. English trade with Wallachia, from the Baltic or the Levant, dried up in the period under discussion. Transactions with Moldavia and Transylvania were directly in the hands of non-English merchants. Finally, the market for England's chief export commodity, cloth, was an extremely limited one, consisting of the few towns of the region and among a few highly placed families.

JAMES E. FARNELL
Roosevelt, New Jersey

ERIC KERRIDGE. *The Farmers of Old England*. Totowa, N.J.: Rowman and Littlefield. 1973. Pp. 180. \$10.00.

This is an important, perhaps even a revolutionary book. Professor Kerridge believes that

the English agricultural revolution was pretty well completed before 1760, the traditional date for its beginning. He has written three books to support his views. The very first sentence in his most important work, *The Agricultural Revolution*, published in 1967, reads: "This book argues that the agricultural revolution took place in England in the sixteenth and seventeenth centuries and not in the eighteenth and nineteenth." The volume presently under review, *The Farmers of Old England*, contains much that is repetitious of the earlier book, but is a slighter work and attempts to be more popular. Less than half the length of the earlier work, it contains no footnotes or bibliography, both of which are very impressive in *The Agricultural Revolution*. The present work devotes more space to the way of life of the actual farmers and to the social and economic impact of the technical changes.

The heart of Professor Kerridge's thesis is found in his chapter entitled "The Great Inventions." Most important was the introduction of "up-and-down husbandry," which "consisted essentially in alternately ploughing grassland *up* for corn and laying cornland *down* to grass." This change resulted in greatly enlarged crops, and hence great profits, but it took many years and considerable capital. Almost equally important was the "floating of watermeadows," a highly technical process, illustrated by several elaborate diagrams, which made possible much larger sheep herds. The introduction of various new crops and grasses constituted a third major technical change.

These technical improvements were widespread by 1700 and led to specialization in crops and livestock and stimulated manufactures and trade. Technical improvements, specialization, and commerce all involved capital, hence larger farms. Inevitably many small farmers became wage laborers, but Professor Kerridge argues that such changes meant an improvement in the standards of living for the vast majority. English society lacked any rigid castes. Rather it was like moving freely up or down the rungs of a ladder. Professor Kerridge's whole thesis is closely argued and supported by much solid evidence. Its acceptance would mean a drastic revision of the traditional interpretation of the agricultural revolution.

The book is difficult to read, for it is filled with many archaic terms of the agricultural vernacular. A glossary would be useful. The book contains a long chapter analyzing the forty "farming countries" into which the author divides England, and which are determined by

such basic agricultural characteristics as soil, climate, crops, and livestock. The frontispiece is a map showing the "countries."

JOHN G. GAZLEY
Dartmouth College

ERNST SCHULIN. *Handelsstaat England: Das politische Interesse der Nation am Aussenhandel vom 16. bis ins frühe 18. Jahrhundert.* (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, number 52.) Wiesbaden: Franz Steiner Verlag GmbH. 1969. Pp. xi, 390. DM 60.

An amazing feat of compression is represented in this detailed analysis of British pamphlet literature from More through Defoe about trade and the interest of state. Bibliographers should note the 26-page appendix that lists in chronological order the full titles of the relevant pamphlets. The text manages to comment on the views of most of these, as well as on the secondary literature evaluating them. The prodigious compactness of this achievement might make it seem especially irrelevant to criticize the author for what he has not done. Yet the state of scholarly debate about mercantilism makes it necessary to warn that there is less in this book than meets the eye. Mentioned in the study are most of the scholars who have written about mercantilism, but there is too little dialogue about the fundamental issues raised by them. The basic question of the relevancy of mercantile theories when compared to commercial practices is dealt with too slightly by Schulin. The reader will not find in this study a continuation of the great debate about whether or not there ever was a coherent system of mercantilism or only theories rationalizing unsystematic expedients. Therefore, the author is not able to be convincing that it is worth his time and ours to lead us through the ponderous arguments of the pamphlet literature he explicates. His basic thesis about the importance of the ideological debates fails to command compelling interest when compared with the thesis that most of the theories of mercantilism may have veiled an unsystematic maze of vested interest rather than interest of state. Schulin's central claim about the significance of his study is that it traces the common thread of political lobbying by the business interests who, after 1640, had enough confidence in their political influence to feel that they could impose their commercial views on their government. Schulin's Continental perspective is most valuable when he contrasts this

self-confidence with the very different attitudes of commercial classes in other European countries. This comparative approach is most welcome in light of the insular point of view that still characterizes too much scholarship about British history. But a comparative perspective, broader than Schulin's, was brought to the study of mercantilism long ago by Hecksher, Keynes, Schumpeter, de Roover *et al.* Schulin begs the question of major debate with such writers by claiming that his study assumes familiarity with the economic and political history of Britain between 1500 and 1715. But readers well read in those areas will realize the need for more skepticism than Schulin displays about the possible discrepancies between theory and practice. Schulin does provide insightful evaluation of some scholarly controversies that enter his review. The secondary literature is best weighed in his discussions of the free-trade controversy of 1604, of the debate over a rising or falling gentry, of the passage of the Navigation Acts. The author does depend on more than the pamphlet literature itself when he traces shifts in the power base of the landed gentry, commercial classes, and banking interests. In his discussion of these shifts Schulin is concerned with issues other than mercantilism, to be sure. But debate about mercantilism remains fundamental to the materials Schulin chose to survey. He has avoided too many of the issues of that debate.

DAVID CLARK
Hope College

CONRAD RUSSELL, editor. *The Origins of the English Civil War.* New York: Barnes and Noble. 1973. Pp. x, 286. \$12.00.

In this new volume in the Problems in Focus series, specialists set forth trends and interpretations of recent research in their fields and suggest future areas for inquiry. The articles do not deal with the period beyond the outbreak of the war in 1642, nor is the war regarded as inevitable. For the most part the contributors deal with subjects long considered fundamental for understanding the war's origins: government, central and local; Parliament and the king's finances; Puritanism and Arminianism; fear of popery; and two conflicting cultures. There is no article on the rise or fall of the gentry since, as Russell explains in his introduction, "social change explanations of the English Civil War must be regarded as having broken down." He suggests, however, that a profitable area in social history to explore

further would be the growing importance of the upper yeomen and their relation to the gentry.

Unafraid to agree with Clarendon that human beings loom large in historical explanations, these modern historians pay serious attention to the policies, tactics, ideas, behavior, blunders, and misunderstandings of leaders on both sides. Although long-standing causes are not ignored, the discussion begins essentially in 1625, not in 1603 or earlier. Unlike James, who at times realized what the traffic would bear, Charles, from the beginning of his reign, seemed to have no sense of the public pulse and reaction to his policies. His support of Buckingham in a reckless and disastrous foreign war; of Laud in his Arminian policies; and his stubbornness in employing doubtful financial and monopolistic devices and embarking upon an unpopular Scottish war, left the monarchy by 1640 "discredited and isolated," even from "its natural friends." Laud was perhaps more blind and dogmatic than Charles, for he unceasingly pushed Arminian doctrines and practices, unconcerned that he was alienating loyal Calvinist Anglicans, who, as Tyacke convincingly demonstrates in his article, had formed the backbone of the church under Elizabeth and James. "Thus the connection between Puritanism and revolution was largely of Charles I's [and Laud's] making."

If Charles and Laud failed miserably to understand the deep-seated reactions of men of their own time, Tyacke, Clifton, and Thomas succeed admirably in revealing to us how seventeenth-century Englishmen actually felt. Why Arminianism was repulsive to men still loyal to king and church; why popery was regarded as "the debasement of Christ's teaching,"—more damnable than any form of paganism"; why an elite court culture smacking of Catholicism aroused the violent feelings expressed in the Root and Branch petition are topics explored in depth by these writers.

The politics, maneuvers, and ideas of the Parliamentary opposition receive due attention. With the information now available to him from the Bedford manuscripts, Russell brings out the leading role played by Bedford, the patron of Pym.

Russell also emphasizes the significant part, too often neglected, played by the peers. He makes a clear distinction between the more responsible Parliamentary leaders, who recognized the king's financial problems, and the mere critics of Charles in 1628. In 1641 the more responsible leaders, Pym, St. John, and Bedford, desired governmental office "not so

much for changes in the constitution, as for changes in policy." Russell and the other contributors agree that any revolution came not from below, but from above, from the new policies Charles pursued against a conservatively minded opposition. Whether the term revolution should be used before 1642 may be questioned. If, however, their general thesis is correct, as I believe it essentially is (in the short run at least), then it follows that the English situation in this period resembles the continental in some respects, as Eliot points out in his article on England and Europe.

Chapters on Scotland and foreign relations have, as Russell states, regrettably been omitted. Clifton suggests that a new study of foreign affairs in Charles's reign, not undertaken since Gardiner, might well illuminate the timing of local anti-Catholic riots. It is unfortunate that in the planning of the book, more space was not allotted to the years 1640–42. The point is made that the men who assembled at Westminster in November 1640 were staunch monarchists who were desirous of reform, and not of war or revolution. We still need to know, however, "by what political processes" these reformers eventually came to fight the king. Mendle makes a brave beginning to clarify this problem in his discussion of the politics and political thought of these two years. His treatment of the largely traditional political thought is more successful than his handling of politics. Here, perhaps owing to the few pages allotted him, it is difficult for the reader to see a clear picture or analysis. Perhaps we need first, as Russell implies, an up-to-date narrative account of these years.

The articles are uneven, those by Russell, Tyacke, Clifton, Thomas, and Eliot are most rewarding to this reviewer. The other articles, nevertheless, make substantial contributions and must not be overlooked. There should be no doubt after reading this book that the search for the origins of the English Civil War will continue to be a fruitful subject for future research and debate.

MARGARET A. JUDSON
Rutgers University

J. R. JONES. *The Revolution of 1688 in England*. (Revolutions in the Modern World.) New York: W. W. Norton and Company. 1972. Pp. xx, 345. \$11.95.

Recent books by John Carswell and J. R. Western have signaled a new interest in the Revolution of 1688, which for the past twenty

or thirty years has been played down in favor of the Great Rebellion and Interregnum of 1640-60 (called by many a "Revolution," though none of them have been able to justify the use of this term). But the English-speaking peoples on both sides of the Atlantic owe their present constitution and their frame of politics to the men of 1688, not the men of 1642, or 1649. Professor Jones makes this point with considerable emphasis, and though it may seem obvious enough, to some it is clearly not, and it is agreeable to have it restated so authoritatively and cogently.

However, his main interest lies not in the aftermath of the revolution, but its actual mechanics, and his chapters on the working of politics, the first year of the reign and the Catholic problem are fresh and stimulating, though they need to be compared with Professor Western's account of the same matters, and the last might be modified and expanded in the light of John Miller's new study of English Catholicism in this period. On the key question as to whether James would have succeeded in securing an amenable Parliament had William not invaded he is more optimistic than most of his predecessors. Again, his portrayal of the king as an energetic and competent electioneer is provocative and stimulating, but in view of the defective nature of much of the evidence and the fact that on this unresolved question many contemporaries were of two minds, his conclusions are rather overstated. Nevertheless, he is right to try to dispel some of the anticipatory gloom with which most historians have approached this monarch's activities.

His general interpretation, particularly of William's motives, is firmly Whiggish and conventional, as was Western's. They are probably right, though again the evidence is not such as to permit any final verdict, and both, I think, play down too much the dynastic motive that was a very important element in William's thinking at all times. Certainly it is difficult to understand Jones's animus against Lucille Pinkham. Her neo-Jacobite interpretation was certainly exaggerated, but it is by no means entirely untenable, and her work should not be neglected by any serious student of the revolution.

J. P. KENYON
University of Hull

WOLFGANG JÄGER. *Politische Partei und parlamentarische Opposition: Eine Studie zum politischen Denken von Lord Bolingbroke und*

David Hume. (Ordo Politicus, number 15.) Berlin: Duncker & Humblot. 1971. Pp. 296. DM 56.60.

Historians and theorists of parliamentary government have found it easy to read back the last century's developments too far into the past. Dr. Wolfgang Jäger's monograph, which does not seem to have received the notice that it deserves, may be regarded as a corrective to this tendency. He has sifted carefully through the writings of two important pre-Burkeian theorists on his subject and brought their opinions on it into focus.

The first of these, Lord Bolingbroke, does not come off very well under this scrutiny. He is pretty well shown to have been an over-ambitious, violently emotional aspirant to political power, who set himself up as a great theorist, picking and choosing among the ideas of greater minds and providing grist, indirectly, for later thinkers because Montesquieu saw something in his concept of separation of powers. Incidentally, Dr. Jäger suggests that he had little idea of separation of powers as now conceived. H. T. Dickinson, whose biography of Bolingbroke (1970) evidently appeared too late for Dr. Jäger to use, thinks that Montesquieu may have misunderstood him anyway or got the idea elsewhere (p. 306).

By contrast David Hume, when he could free himself from a bent toward utopianism, was a serious and deliberate thinker, and saw things, on the whole, pretty much as they were. Ideally opposed to parties altogether, he recognized them as an indispensable feature of political life and fitted them into a soberly calculated scheme of things, involving a governing king and a regular parliamentary opposition, kept in a minority by the use of influence. His ideas ought to have had an important influence on the course of party evolution, but apparently they received serious attention only from the fathers of the American constitution.

Dr. Jäger has made a distinctly worthwhile contribution. Especially with reference to Bolingbroke, his meticulous survey of the many writings by other authors brings the subject together quite well. He demonstrates that Bolingbroke and his opposition party screamed against Walpole's destruction of the constitution without any clear idea that the House of Commons could be put on a permanent peaceful basis of opposition between "ins" and "outs." Hume, of course, set his mind to working out a practical system of balance and control. But he, too,

had no conception of the opposition as an "alternative government."

What it all comes to is that, although Dr. Jäger supports the theory that thinkers influence not only other thinkers but also the course of events (p. 31), the general tendency of his discussion points in the opposite direction.

CHESTER H. KIRBY
Brown University

JOHN G. GAZLEY. *The Life of Arthur Young, 1741-1820*. (Memoirs of the American Philosophical Society, volume 97.) Philadelphia: the Society. 1973. Pp. xvi, 727. \$10.00.

Every schoolboy knows that Arthur Young started life as a Suffolk farmer, soon went bankrupt, and thereafter made his living and reputation by instructing Englishmen on how to succeed at farming. Among the valuable achievements of Professor Gazley's definitive biography, dispelling this myth is a notable one. Young became the squire of Bradfield Hall, Suffolk, in 1785 and stayed there until his death in 1820, cultivating a large farm himself and renting two farms to tenants. Bradfield thus remained the cherished center of an extraordinarily active life that took him, as an itinerant agronomist, to all corners of the English countryside as well as to Ireland and France and, as secretary of the Board of Agriculture, regularly to London.

What historians, especially agricultural historians, will miss in Professor Gazley's book is an assessment of Young as an agronomist. His agricultural writings are usefully summarized, including his many contributions to the *Annals of Agriculture*. From this it is clear that Young was a skilled and indefatigable publicist. But it is less clear how adequate he was as an agricultural expert. Some modern agricultural historians, notably Eric Kerridge, have a low opinion of Young's technical competence; they take sides with his contemporary and rival, William Marshall, whom they rank higher than Young. The merits of their case might profitably have been examined.

On the other hand Professor Gazley's wonderfully detailed account of Young's personal life more than compensates for this omission. In a labor of many years he has sought out every trace of his hero. What impresses most in his evocation of the man is Young's obsessive quality. He had more than his share of disappointments and tragedies: marital wretchedness, a cantankerous son, the untimely death of a be-

loved daughter, and his own blindness in his later years. But he never faltered: the flood of written words went on almost to the end, as did his attendance at the Board of Agriculture. He was the sort of man who, in winter as in summer, rose at four o'clock in the morning and plunged into a nearby pond. On his wife's tombstone he inscribed that she was the daughter of John Leigh of Norfolk, the first "who there used marl." Possibly he had nothing else to say about the poor woman; on the other hand he was not likely to miss a chance to celebrate the glories of agricultural innovation.

DAVID SPRING
Johns Hopkins University

The Correspondence of Edmund Burke. Volume 9. Part 1, *May 1796-July 1797*, edited by R. B. MCDOWELL; part 2, *Additional and Undated Letters*, edited by JOHN A. WOODS. Chicago: University of Chicago Press. 1970. Pp. xxviii, 487. \$21.50.

With the exception of an index volume the Burke correspondence is now complete. Scholars who have used previous volumes will find this volume familiar, although of less value than some of the earlier ones. Now in declining health Burke still delivers his opinions on topics that interest him, especially Ireland and revolutionary France, with his usual vigor and point of view. Since Burke was no longer directly involved in public affairs the letters have little to contribute to the history of the period. A few reminiscences, especially of his role in economical reform, shed light on the great events in which he was once engaged.

Presumably this review concludes a series that began with Walter Love's review of volume 1 (*AHR*, 64 [1959]). In his review Love praised the high quality of the editing and notes and looked forward to the scholarly benefits expected to flow from a complete edition of several thousand Burke letters, most of them unpublished. Now that the series is complete it is clear that the highest editorial standards have been maintained throughout, but the value of the correspondence to the historian is less obvious than it seemed to Walter Love in 1959. The editors have cleared up a mass of detail about Burke's personal and family life, but Burke's public career and private affairs were in such incongruous contrast that one wonders what contribution this mass of biographical information can make to historical understanding. The Burke correspondence adds many bits and pieces to the political

history of the period, but it does not bring important new insights nor does it open the door to significant new studies. The correspondence contributes something to understanding Burke's positions on the great issues of his time—America, Ireland, India, reform, the French Revolution—but the major sources for his ideas on these topics are still *The Parliamentary History* and his *Works*. On Burke's development as a political philosopher the correspondence has little to contribute. Professor Copeland and his dedicated crew have done their work well and have elucidated many aspects of Burke's long and active career, but it is difficult to see what new impulse the completion of the correspondence will give to studies of Burke and his time. Rather, this superb edition of Burke's correspondence may well mark the culmination and conclusion of the "Burke revival" that began about twenty-five years ago.

E. A. REITAN
Illinois State University

MICHAEL BROCK. *The Great Reform Act*. London: Hutchinson University Library. 1973. Pp. 411. Cloth £4.50, paper £1.95.

Michael Brock's study of the 1832 act and the events that led up to it has been over a decade in coming, but it is worth the wait. Not that Mr. Brock has anything new to say. Neither G. M. Trevelyan nor J. R. M. Butler would find much to quarrel with in this volume, but for the last twenty years or so the conclusions of the so-called Whig historians have been out of fashion. A new study was necessary to vindicate the good sense of the past over the nonsense of the present. Mr. Brock does just this, brilliantly and exhaustively.

The first chapters deal with the disintegration of the old political system after the death of Lord Liverpool and the growth of pressure for reform, which began in earnest with the general election of 1830. The mounting agitation after the election and the influence of the July Revolution and of industrial and agricultural unrest are carefully traced. Mr. Brock leaves us in no doubt that public agitation was critical in launching the bill and in every stage of its subsequent history, nor that its Whig framers saw it as a necessary concession to public opinion. Equally clear is that the Whigs firmly believed that future political stability depended upon appeasing the middle classes and bringing them within the political system. The Whigs also believed that the result

of such reform would be to re-establish the authority of the natural leaders of society, people like themselves. Reform would restore confidence in the political system and allow aristocratic administrations to practice those arts of government that only they fully understood.

Were the Whigs right? This is a very difficult question, and perhaps not one to be answered by the historian of the Great Reform Act. The pressures that Grey's government perceived acting upon them and the solutions they devised are one question, the effects of the resulting act another. Mr. Brock has exhaustively investigated the first question. If he has missed a significant monograph or an important collection of manuscripts, it is one unknown to this reviewer. Few probably would quarrel with his broad conclusion that the act was a crucial determinant in the relatively peaceful progress of political reform in Victorian Britain. But why this was so will probably be debated for some time to come. Mr. Brock seems to believe that the answer lies in the re-establishment of a deferential attitude among the middle-class electorate. But the Whigs never talked of deference, but rather of the influence of property. It is unlikely they ever expected the new electorate to defer to that influence where its own immediate interests and prejudices were involved. They may have been surprised by the extent to which postreform politics were dominated by just such questions.

RICHARD W. DAVIS
Washington University

DAMIAN MCELDRATH, O.F.M. *Richard Simpson, 1820-1876: A Study in XIXth Century English Liberal Catholicism*. (Bibliothèque de la Revue d'Histoire Ecclésiastique, number 55.) Louvain: Bureau de la R.H.E., Bibliothèque de l'Université. 1972. Pp. xix, 163. 200 fr. B.

FREDERICK J. CWIEKOWSKI, S.S. *The English Bishops and the First Vatican Council*. (Bibliothèque de la Revue d'Histoire Ecclésiastique, number 52.) Louvain: Bureaux de la R.H.E., Bibliothèque de l'Université. 1971. Pp. xxvii, 341. 500 fr. B.

These books are works of solid scholarship. Damian McElrath has given us a portrait of Richard Simpson, the man, as well as a more complete account of Simpson's liberal Catholicism than heretofore available. Next to Acton he was the leading figure of the English liberal Catholic movement of the mid-nineteenth century. In addition the versatile Simpson was a

competent historian and Shakespearean scholar. His wide range of interests included theology and philosophy, geology, the classics, and architecture. Cwiekowski's book is a detailed study of the English Roman Catholic bishops at Vatican I. Both scholars have made good use of unpublished sources in relevant archives and manuscript collections. Among other sources Cwiekowski drew material from various diocesan archives in England.

McElrath's book begins much better than Cwiekowski's. He leads the reader right into his subject, whereas the first section of Cwiekowski's introduction, "England in the Nineteenth Century," seems unnecessary for the range of readers likely to be attracted to his subject. The early chapters in McElrath's book are among the most interesting. He relates Simpson as a young man to Simpson later in life as a liberal Catholic; for instance, a connection is made between Simpson's lack of moderation in his essays as an undergraduate at Oxford and his later writing for the liberal Catholic periodical, the *Rambler*. McElrath uses, among other sources, manuscript material in the public library at Mitcham (where Simpson was raised and where he became an Anglican vicar before his conversion to Roman Catholicism) to provide interesting new background and detail about him. He emerges from the pages of McElrath's book as an attractive personality, albeit one with considerable pugnacity in controversy. John Henry Newman and Simpson disagreed over the latter's theological writing in the *Rambler* and some other matters, but they remained on cordial terms and regarded each other highly. Again, despite many disagreements with W. G. Ward, a leading ultramontane, Simpson personally liked him. And notwithstanding their squabbles during their association with the liberal Catholic *Home and Foreign Review*, the successor of the *Rambler*, T. F. Wetherell and Simpson respected each other. So, too, Cwiekowski indicates that disagreements in ecclesiastically related matters did not necessarily preclude feelings of respect. Although Bishop T. J. Brown of Newport had delated Newman's article, "On Consulting the Faithful in Matters of Doctrine," to Rome, he later spoke highly of Newman's *Letter to Pusey* (1866), and he wanted Newman to attend the Vatican Council.

One wishes McElrath had elaborated upon the following statements he made: "Their [Acton's and Simpson's] Liberal Catholicism anticipated by a hundred years present currents within the Roman Catholic body" (p.

xviii) and "It is surprising [*sic*] how many of the questions over Catholic issues undertaken by the Liberal Catholic Simpson, convert and layman, anticipated by a century debates presently raging in the Roman Catholic body between liberals and conservatives. In fact, some issues already anticipated by Simpson have yet to appear in the contemporary arena" (p. 157). But one wishes, too, that the expression, a "siege mentality," had not been used several times in the book; it has become a cliché.

Cwiekowski's study shows clearly that the extreme position of Archbishop Manning regarding the pope was not representative of the English bishops generally. Most English bishops were among the moderates and the minority at the Council. Besides Manning, a main figure at the Council, Bishop Ullathorne of Birmingham and Bishop Clifford of Clifton emerge as the most significant English bishops at Vatican I. Ullathorne, ultimately a moderate member of the majority, is shown to be a man of good sense and practical ability. Clifford was an active figure in the minority, many of whom opposed a definition of papal infallibility as inopportune. Cwiekowski describes the English bishops' involvement in the work of the Council, debates in the general congregations, deputations, suggested revisions of schemata, and so forth. Besides various archival materials, he apparently knows the published works relevant to his subject. Though his knowledge is impressive, the book seems somewhat too heavy with details; a judicious pruning would have been useful. With its complex details, sometimes bulky footnotes, and lengthy quotations some readers may feel a bit of the tiredness that bishops must have felt as the days and months at the Vatican Council went by. It would have been an aid to some readers to provide English translations of the quotations from texts and speeches in Latin at the Council.

Both McElrath and Cwiekowski have striven to give fair and balanced views of their subjects, and they have largely succeeded. McElrath obviously admires Simpson, but he is critical where need be. Cwiekowski gives a scholarly, fair-minded presentation of the English bishops at the First Vatican Council and of Acton, Newman, and others concerned.

WILLIAM J. SCHOENL
Michigan State University

DENNIS G. WIGMORE-BEDDOES. *Yesterday's Radicals: A Study of the Affinity between Unitarianism and Broad Church Anglicanism in the Nineteenth Century*. Foreword by ALEC

VIDLER. Cambridge: James Clarke and Company. 1971. Pp. 182. £2.10.

An apologetic purpose informs Mr. Wigmore-Beddoes's little essay: to demonstrate the affinity between liberal Anglicans and Unitarians a hundred years ago may clear Unitarians today from the label of heresy and so help to restore the influence they have lost in this century. One wonders if a present-minded activity like theology today will be any more affected by historical demonstration than, say, politics is. Even granting that, would much positive result emerge from Mr. Wigmore-Beddoes's wistful descriptions of the influence of Coleridge, Blanco White, and James Martineau on a few Anglican contemporaries?

But the book is less about apologetics than about history, and here there are questions, too. The chapter titles are indicative: "The Affinity shown" with respect to higher criticism; biblical inspiration, miracles, everlasting punishment, the atonement, and Christ's divinity; traditional language, liturgical practice, and architectural style; mutual interaction; and the idea of a broad church. We are shown briefly, and not unusefully, what leading (not necessarily representative) Unitarians thought on these subjects and then what leading Broad Churchmen thought—a *catalogue raisonnée* with no surprises; the common characteristics are seen in the end as a commitment to reasonableness, a desire for comprehension, and a similar "moral and religious sensitivity." In such a catalog so much must be compressed that disagreements about interpretation are inevitable—a broader look at stylistic development or a study of the commissioning of particular churches would, for example, almost certainly alter what is said about architecture. But a more serious difficulty lies in the limitation of the study largely to the generation of James Martineau. Thus, like most Unitarian historians, Mr. Wigmore-Beddoes misses the strife-torn development of Unitarian thought in the nineteenth century—and so the perspective needed to measure its "radicalism," its relevance, and its influence.

R. K. WEBB

American Historical Review

PHILIP WARNER. *The Crimean War: A Reappraisal*. New York: Taplinger Publishing Company. 1973. Pp. 232. \$9.95.

R. L. V. FRENCH BLAKE. *The Crimean War*. (Concise Campaigns, 1.) [Hamden, Conn.: Archon Books. 1972. Pp. x, 181. \$9.50.

The Crimean War is enjoying a revival triggered over two decades ago by Cecil Woodham-Smith's *Florence Nightingale* and the more sensational *The Reason Why*, recently made into the movie *The Charge of the Light Brigade*. In 1961 Christopher Hibbert produced *The Destruction of Lord Raglan: A Tragedy of the Crimean War* and in 1970 came John Selby's well-illustrated *Balaclava: Gentleman's Battle*. In 1968 the memoirs of surgeons George Lawson and Douglas A. Reid were made available, to mention but a few items. Thus the question arises, do we need any more books on this limited mid-nineteenth-century war between the great powers? The answer is yes only if they really have something to contribute to our knowledge of the conflict. In this case the answer is basically no.

This is not to say that these books are not useful popular accounts that undergraduates or ROTC students will not find enjoyable; it is to say that for scholars they really tell us nothing we do not already know. Philip Warner of the Sandhurst establishment labels his work a reappraisal. It is a lightweight attempt to put the army in perspective and to rebut Mrs. Woodham-Smith, while at the same time placing the medical work in its proper place within the history of the development of the health services. The select bibliography makes no mention, however, of medical literature or even of the official medical reports that so influenced the Union medical service in the American Civil War. Colonel French Blake's account is really the more useful since it is written strictly from the military point of view and pays attention to tactical affairs with excellent maps and timetables of the battles, as well as providing marked battle panoramas, a list of regiments of the British army by number and names, and a reasonable bibliography. If French Blake is representative of the new series, Concise Campaigns, then the military historian has something to look forward to at that level.

ROBIN HIGHAM

Kansas State University

PETER HARNETTY. *Imperialism and Free Trade: Lancashire and India in the Mid-Nineteenth Century*. Vancouver: University of British Columbia Press. 1972. Pp. ix, 137. \$7.00.

FRANCIS E. HYDE. *Far Eastern Trade, 1860-1914*. (The Merchant Adventurers.) New York: Barnes and Noble. 1973. Pp. xii, 229. \$11.50.

Professor Peter Harnetty has written about still

another aspect of the relationship between free trade and British imperialism in the nineteenth century. As is well known, the classical view of this relationship, maintained by Hobson, Schumpeter, and Lenin (as well as W. L. Langer, R. L. Schuyler, and, most recently, D. C. S. Platt), was that free-trade doctrine was at the heart of mid-Victorian indifference, even hostility, to empire. On the other hand, J. Gallagher, R. Robinson, and others have pointed to the reality of imperial expansion and exploitation during this period when statesmen mouthed the rhetoric of anti-imperialism, and D. N. Winch and I saw in the character of classical economic theories a stimulus to imperialism. Albeit on a free-trade base, an industrial Britain was persuaded that it needed to establish commercial ties of an essentially mercantilist character with agricultural nations that would supply England with raw materials, markets for her surplus manufactures, and profitable sites for investment. In the early decades of the nineteenth century a number of economists, most prominently F. List in Germany and H. C. Carey in the United States, feared that without tariffs to protect the growth of domestic industry, their countries would be reduced to "informal" colonies of a British commercial and industrial metropolis.

In Britain's economic relations with India we recognize, in the heyday of Cobdenism, the survival of a mercantilist imperialism. Unlike the German states and America, as Harnetty tells us in his monograph, India could not protect herself against the low prices of British textiles that threatened the ruin of the native cotton industry, for the imperial metropolis deliberately intervened to prevent the colony from erecting tariffs against the manufactures of the mother country. Since the British workshop of the world had no reason to fear competition, the true character of this policy was somewhat hidden from contemporaries because it masqueraded as devotion to a cosmopolitan free trade.

Moreover, as Harnetty demonstrates, the Lancashire cotton manufacturers, who at home marched beneath the banners of *laissez faire*, persuaded the British government to adopt an interventionist policy in India. In the interests of Lancashire the government of India guaranteed the profits of railway construction and financed public works to facilitate English commercial penetration, as well as actively working to improve the cultivation and marketing of Indian cotton. Harnetty has made a short but useful contribution to the shamefully

neglected field of the nineteenth-century British economic relationship with India. A perhaps minor cavil might be that a book on economic policies should devote some attention to the economic theory of the time.

India, whose offer of opium and cotton for Chinese tea and raw silk was the opening wedge in Britain's trade with the Far East, plays only a peripheral role in Professor Francis E. Hyde's account of Far Eastern trade between 1860 and 1914. Just as Britain in the first quarter of the nineteenth century was moving toward free trade, Holland, which had previously administered her colonies more or less on that basis, revived a mercantilism that may have served as a model for the British in India. By the middle decades of the century a growing Dutch liberalism was to undermine this policy, thus helping British and Malayan Chinese merchants to break the commercial monopoly of Batavia for the distribution of Indonesian produce in favor of British Singapore. Hyde demonstrates that of all the European countries, Britain appears to have been the chief beneficiary of Asian trade, especially of trade with China.

The development of the mainline steamship companies between Europe and East Asia occupies a central role in Hyde's story. Along with the growth of telegraphic communication, this made possible an expansion of trade with Europe as well as the stimulation of local trading. Great entrepôts, with their rich merchants' houses and banks, sprang up at Hong Kong, Singapore, Yokohama, and Shanghai, altering the character of the traditional trade. The production of the traditional Chinese staple of tea was captured by India and that of silk by Japan. Western capital, soon supplemented and to some extent replaced by indigenous capital, mostly supplied by the resident Chinese merchants of Southeast Asia and the Japanese traders in China, promoted the production of Indonesian coffee, sugar, and tobacco, Siamese rice, and Malayan rubber and tin. Once brought out from its traditional, xenophobic isolation into the arena of world trade by the force of Western capital and technology, the Far East did not long remain a mere offshoot of the Western industrial economy, but became the hub of a Pacific economy in which Japan was to play the dominant role. Hyde has written a straightforward, valuable study.

BERNARD SEMMEL

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ROBERT G. GREGORY. *India and East Africa: A History of Race Relations within the British Empire, 1890-1939*. New York: Oxford University Press. 1971. Pp. xviii, 555. \$24.00.

Asians have occupied a pivotal place in East Africa's twentieth-century history. Asian laborers helped to build the railroad across East Africa. Asians constituted the dominant commercial class, spreading the new money economy into remote African areas, and they organized early and influential political associations. At a time when this community's very existence is threatened it is important to have a serious historical study of the origins and growth of the Asian population, such as Robert G. Gregory's *India and East Africa*. Despite its all encompassing title this book basically seeks to describe how the Asian immigrant community sought to make a place for itself within the British Empire between 1890 and 1939 by mobilizing support for its cause, not only in East Africa itself, but among Indian leaders in British India and humanitarian and colonial circles in Great Britain. The most salient feature of this book is its exhaustiveness of archival research. Drawing data from the Colonial Office archives in London, the India Office, and the Kenya colonial government archives, Gregory provides a fuller story of the Indians and the development of colonial policy toward the Indian population than we have previously had. Most East African history has been written on the basis of London and Nairobi records; Gregory's use of New Delhi material provides fresh new insights and dimensions on Indian leaders in East Africa, men like Isher Dass and M. A. Desai, as well as on Indian nationalists like Gandhi as they developed policies toward the British colonies in East Africa. Although the author does not develop radical new interpretations of East African history, he does show in great detail how the India Office, spurred on by its problems of administering a colonial society already troubled by anticolonial nationalism, took an interest in Indian problems in East Africa and sought to ameliorate the economic and political conditions of that population. Quite naturally, the book focuses on Kenya, not only because the Indian population there was larger and more dynamic but also because the most explosive racial questions were fought out in that troubled colony. Much less serious consideration is given Uganda, Tanganyika, and Zanzibar, although all three countries are discussed.

Despite the obvious achievements of this work and the commitment to collecting full data there are certain flaws in the author's organization and presentation. Most of these stem from too great a reliance on official British sources, especially India Office and Colonial Office documents, for assembling a portrait of the Indians. These documents tend to give a rather detached view of the Indians and often fail to convey a clear understanding of the aims, ambitions, and driving motives of this admittedly diverse people. As a result the author displays a tendency to depict the Indians as constantly reacting to the initiatives of others, usually European settlers and the British Colonial Office. To be sure, the Asians did not have political power, but they enjoyed more autonomy within certain limited economic and social spheres than the author suggests. Moreover, because of his reliance on official sources, Gregory structures his narrative around issues the British regarded as important, but to which the Indians sometimes did not attach the same immediacy. This mode of presentation works well enough for the events of the 1920s when critical questions of landholding, voting rights, and immigration exercised Indians as fully as the British. But it is not so successful for the more diffuse events of the 1930s. To take but one salient example from the 1930s, Gregory discusses at great length the influential Kenya Land Commission while relegating to a subordinate position a discussion of changes in marketing procedures. The land settlements suggested by the Kenya Land Commission were indeed critically important for Europeans and Africans, and they were symbolically significant for the Indians because they reaffirmed the exclusion of the Indian population from landholding rights in the so-called white highlands of Kenya. But commerce was the life-blood of the Indian population; changes enacted in the marketing of African produce in 1935 were seen as jeopardizing the very existence of the Indian population. Thus Indian delegates to the Kenya Legislative Council walked out in protest over *this* question, not over the findings of the Kenya Land Commission.

This flaw in perspective notwithstanding, *India and East Africa* is an extremely important, well-researched study that provides new perspectives for viewing the making of policy in twentieth-century Kenya.

ROBERT L. TIGNOR
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OZER CARMI. *La Grande-Bretagne et la Petite Entente*. (Travaux d'histoire éthico-politique, 24.) Geneva: Librairie Droz. 1972. Pp. 377.

The Little Entente has bulked larger in the consciousness of historians than it did in calculations of major Foreign Offices at the time. The British, whose attitudes from 1920 to 1937 are chronicled here in abundant detail, were relatively indifferent to political arrangements in the Danube basin. Britain's one vital interest was to promote the region's economic rehabilitation in order to preserve the market for British goods and capital.

Carmi appears to recognize the fragmented and essentially parochial nature of his subject, though, as is only natural, he prefers not to call attention to its limited significance explicitly. In fact, the other great powers took the Little Entente hardly more seriously than did England. The Germans, who remained economically dominant in the area throughout, never considered the formal arrangements among the Danube states more than a subsidiary obstacle to their own ambitions. Even the French, as the newly opened archives confirm, always remained properly skeptical of the concrete military worth of their Eastern alliances.

The members of the Little Entente themselves were bound together by a common fear of regional Hungarian revisionism, not by wider European concerns. Hungary aside, the Romanians were worried primarily about Russian and Bulgarian designs; the Yugoslavs about Italian ambitions; the Czechs, at least until the 1930s, more about the Polish and Russian than the German danger. A firm basis for united action was thus often wanting.

Nevertheless, Carmi strains to portray the smaller nations of East Central Europe as continually at the center of diplomatic action. His narrative of relations between Great Britain and the Little Entente in regard to a variety of issues in which the Danube nations played a peripheral role—say the Four Power Pact of 1933 or the Italo-Ethiopian War—at least has the merit of unusual perspective. However, his geographical focus does lead to frequent distortion. Carmi contends, for example, that negotiations for a British guarantee of French security broke down in 1922 primarily because France insisted on analogous assurances for its East European allies, when the stumbling block actually proved to be Britain's unwillingness to make a precise military commitment to the French frontier. Similarly, Carmi seriously overstates the degree to

which France, no less than England, was preoccupied with East Central European problems at the time of Locarno and during the Danube customs union negotiations of 1932. The collapse of the Little Entente, at the first sign of real danger in 1936–37, should have led the author to reflect upon the hollow nature of that alliance earlier.

Carmi's scholarly apparatus is flawed. His monograph is based primarily on five hundred volumes of Foreign Office political correspondence, but these evidently have been sampled rather than studied exhaustively. No less than thirty-seven volumes actually deal (according to Public Record Office directories) with countries other than those under which they are listed in the bibliography. Carmi makes no use of Cabinet or other British primary materials. Nor has he examined either the French or German archives, or the copious secondary literature in German, which includes Günter Reichert's study of the collapse of the Little Entente.

STEPHEN A. SCHUKER
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NIGEL HARRIS. *Competition and the Corporate Society: British Conservatives, the State, and Industry, 1945–1964*. [London:] Methuen and Company; distrib. by Barnes and Noble, New York. 1972. Pp. 384. \$12.75.

The focus of this study is the adjustment of the Conservative party to the changes in British society of the last several decades, but particularly since the end of World War II. The author seeks to show how the Conservative party has been able to maintain its position as one of the most successful political parties in modern history, and to this end he is more interested in describing the party's response to changing conditions than in assessing the validity of the response.

Insisting that the party has no "theory" of the status quo, and rarely needs one, Harris stresses that what the Conservatives defend at any given moment depends upon the balance of forces within the party and on what is being attacked by opposition parties. Indeed the party resists theorization because a consistent theory would threaten the coalition of contradictory interest groups that is the core of the party's strength. Nevertheless, it is possible to construct a hypothetical Conservative theory, and this theory, according to the author, is a blend of "etatist corporatism," more or less opposed to liberal ideology, and "pluralist

corporatism," the latter largely a legacy of the liberals. Much of the party's rhetoric deals with the free market version of political economy, but over time, Harris argues, there has been a decisive shift toward acceptance and even expansion of state planning and the role of large-scale corporate enterprise. Hence the socialists were right, he suggests, to identify the overall process of capitalism's history as quasisocialist.

The new order of the Conservatives, however, was created not as a response to popular demands, but as a result of involuntary changes in the structure and needs of corporations and shifts in the foreign environment. Nevertheless, fragments of a commitment to free enterprise coexist with attempts to regulate or suppress competition and extend state ownership. Both positions represent different elements in party history, and while the resulting policy-mix would frequently appear to be muddled and ambiguous, a clearer and more consistent result would precipitate open conflict within Conservative ranks. In short, the Conservative party survives not despite but because of its lack of principle and refusal to espouse any coherent political philosophy.

Of course much of the same statement could be made about the Labour party or, for that matter, almost any modern democratic political party. The only ideological parties these days are to be found in authoritarian societies and police states, and more and more often not even there. The conditions that make for flexibility and compromise are present everywhere, and certainly Harris is correct to forecast that the next major challenge to the British Tories will be the adjustments that are made necessary by the changing patterns of international business and the multinational corporation that already dominates the European Common Market. Such adjustments are not made easily, as witness the fumbling of the Heath government, but the opportunism and resilience of the British Conservative party, if the long past is any guide, should never be underestimated.

ARNOLD A. ROGOW
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L. M. CULLEN: *An Economic History of Ireland since 1660*. New York: Barnes and Noble, 1972. Pp. v, 8-208. \$8.50.

Dr. Cullen has provided a wide-ranging survey of the economic history of Ireland. In fulfilling his aim to outline its main features since

1660 he has covered more than 300 years, during which Ireland has progressed from an agricultural country in the mid-seventeenth century to a point where Ireland's interest today lies in stable international conditions, a rising level of world economic activity, and ready access for expanding exports to foreign markets. Ireland has gone through repeated cycles of stagnation and prosperity with periods of famine, high emigration rates, and distressed living conditions as well as those periods when the country's economy flourished. All aspects—agriculture, industry, export-import trade, employment and unemployment, banking and currency, living conditions, transportation, population, prices, etc.—are touched upon. As might be expected in a small book (181 pages) details on any one subject are limited. Because of the many topics discussed it is frequently difficult to follow the course of a given industry or phase of the economy during the three centuries.

Froude and Lecky, writing in the second half of the nineteenth century, set the pattern of considering Irish economic history in the context of politics and legislation. As a result there had been a general acceptance of the view that the economic background was reflected in the two dominant political themes: the land question and home rule. Cullen asserts that the modern dilemmas—political and economic—are less intertwined in their historical backgrounds than has often been assumed. He feels that economic interests are not the chief cause of political conflicts today; the political, religious, and racial forces have had great strength in their own right in Irish history.

Throughout the book Cullen attempts to explode long-standing ideas about Irish backwardness and stagnation. His views are contrary to those of many previous writers on such subjects as the potato, the long dependence of Ireland on exports, the unduly dark picture of Irish social conditions in the prefamine decades, the attitudes toward landlords, and the economic vitality of postfamine Ireland. Future historians will find much to accept and expand or to challenge. An absence of any footnotes will make it difficult to check with ease the specific data that are given. The author has provided, however, notes on the many primary sources that he has used.

Since Irish economic history as a study is still in its infancy Cullen's book may be expected to remain a standard work for many years. Based on extensive research in archives pertaining to economic and social conditions in

Ireland it not only provides a good introduction to the development of Ireland's economy, but will serve as a reference for those investigating areas that are not yet fully explored.

HOMER L. CALKIN

U.S. Department of State

DONALD HARMAN AKENSON. *Education and Enmity: The Control of Schooling in Northern Ireland, 1920-50*. (A publication of the Institute of Irish Studies, The Queen's University, Belfast.) New York: Barnes and Noble, 1973. Pp. 287. \$13.75.

It is difficult to summarize this book because Professor Akenson has chosen a chronological approach that does not give the reader enough generalizing statements to carry him along and help him get his bearings.

Akenson seems interested mainly in the primary school system in Ulster, but he remarks on intermediate, technical, and grammar schools as well. The book might have been easier to follow if the author had focused exclusively on the primary schools and left the rest of Ulster's educational "old curiosity shop" alone.

To summarize the argument about primary education, the government of Northern Ireland inherited from the British government a primary school system that was in fact denominational (that is, Catholic and Protestant schools controlled by the respective clergies) even though it was financed largely by the state. In the early 1920s the Stormont government required that any primary school must, to qualify for full public funding, come under the jurisdiction of regional and borough educational committees. Since those committees, given gerrymandering in Ulster, were dominated by Protestants, the law was anathema to the Roman Catholic hierarchy, and Catholic schools remained "voluntary" schools that were funded only in part by the state. From the first, then, the public school system was Protestant in clientele. The Protestant clergy soon succeeded in making them Protestant in doctrine as well by putting pressure on Stormont to make "Bible training" obligatory in public schools. This made the Catholic hierarchy even more reluctant to have their schools become part of the public system. Thus evolved the system of separate and unequal schooling in which the primary schools have been "twisted into Protestant institutions fully dependent upon public funds" while Catholic schools get less than full public funding.

Akenson's account is scrupulously even-handed. He calls attention to an interference in politics on the part of the Protestant clergy that far exceeds that of the Catholic clergy in "the former United Ireland," even though Protestant clergymen have ritualistically denounced "priests in politics." But he shows that the Catholic bishops and priests are also responsible for the unequal status of Catholic schools. He makes the telling argument (p. 116) that the law could have been manipulated so that Catholic schools could have received full public support while remaining Catholic in clientele, teaching staff, and doctrine if the clergy had been willing to admit the laity into the management of their schools. But the clergy were determined to keep control solely in their hands, thereby perpetuating "the usage of inadequate and outmoded school facilities." It would take a Voltaire to exploit the ironies in this account of Protestant and Catholic sins committed in sweet Jesus' name.

Though unequal, the Catholic schools were not all that unequal. The state has from the first paid teachers' salaries and has raised building grants from fifty per cent in 1930 to eighty per cent in 1968 (though Catholic schools must change their status from "voluntary" to "maintained" to be awarded eighty per cent). This amount of state support would turn an American bishop green with envy. Northern Ireland, then, has not been merely the instrument of the Protestant clergy in educational matters. Unionist politicians have made progressively larger grants to the Catholic schools in the hope, which the British raj also entertained until 1921, that concessions to the bishops would persuade them to keep the natives quiet.

The major criticism of the organization of this book is that Akenson ends the account at 1950. Have there been no educational developments since then that are worthy of close examination? To choose 1950 as a cutoff date is even more baffling since Akenson makes, in his concluding chapter, suggestions for reform of the system as it stands today. And one can criticize his assumption that education has that great an influence on enmity between Protestants and Catholics. As he notes, the family and peer group are far more important in shaping the attitudes of Protestants and Catholics toward one another. Akenson recommends that Catholic schools be fully supported by public funds if the clergy agree to admit laymen to a larger share in school management—a big if (as he realizes). This proposal may be more feasible than his sug-

gestion that experiments in integrated schooling be made in the hope that they might "reduce ill-feeling and misunderstanding" between the two communities, and he specifies the ideal conditions under which integration should take place. Such a recommendation seems to assume radical changes in Protestant-Catholic relations if it is to be workable. Akenson furthermore ignores the psychological advantages that follow from a segregated system. As Rosemary Harris points out in her excellent study, *Prejudice and Tolerance in Ulster* (1972), segregation in education and in every other social activity endows the individual Protestant and Catholic with a strong sense of personal and group identity. There is no generation gap; parents communicate with their children because they (the parents) are anxious that the children remain with their own kind and keep the faith. This sense of belonging may be more valuable than whatever integrated schooling might achieve.

Akenson is to be congratulated for having taken on so complicated and controversial a subject and for reading through statutes, debates, and reports that are not only technical and dull but depressing in their revelation of selfishness and self-deception. His book will be a standard work of reference for historians interested in Northern Ireland.

JOSEPH WOODS
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JOSEPH GOY and EMMANUEL LE ROY LADURIE, editors. *Les fluctuations du produit de la dîme: Conjoncture décimale et domaniale de la fin du Moyen Âge au XVIII^e siècle. Communications et travaux.* (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches historiques. Cahiers des études rurales, number 3. Association française des Historiens économiques. Premier Congrès national—Paris 11–12 janvier 1969.) Paris: Mouton & Co. 1972. Pp. 396. 75 fr.

This volume extensively illustrates both the rich potentialities and the practical difficulties that characterize the statistical method that has become the hallmark of the VI^e Section of the École Pratique des Hautes Études. The original proposal that led to this publication was made by Ernest Labrousse who urged that the total agricultural production in France during the early modern period might be measured by examining the relatively voluminous records of the ecclesiastical tithe. Ful-

fillment of this fruitful suggestion clearly required the cooperative effort of many researchers and was undertaken by a group at the above-named institution, with the help of additional personnel. Their initial findings, which they presented to the Congress of the Association of Economic Historians in Paris, January 11–12, 1969, are set forth in this volume, together with an introduction and summary-conclusion by Joseph Goy and Emmanuel Le Roy Ladurie in which the latter synthesizes the group's researches and presents tentative conclusions. The reports contain extensive statistics concerning the fluctuations of the tithe in sixteen different areas from all parts of France, with the notable exception of the west, and trace this source of ecclesiastical revenue with the sophisticated techniques of the school.

This investigation of the tithe in many different areas of France over several centuries inevitably encountered a host of difficulties. First, there were various categories of tithes: those collected by the clergy as a percentage of certain products, and those collected by tax farmers on a single crop or several crops, either in money or in kind. (If in money, the statistics are here converted by a complex process into equivalents in produce.) Then there were innumerable local variables: different rates of assessment and products assessed, and differences in the form, duration, and application of the tax farmers' contracts. Occasionally the tithe was levied merely upon units of land, and certain lands, notably newly cleared acreage, were entirely exempt. Also the collection of tithes was powerfully influenced by such external factors as inflation and deflation, the impact of wars, antitithe agitation especially during religious upheavals, graft and corruption, and demographic changes. Many of these factors may be partially neutralized, however, and a majority of the contributors claim to have established a reasonably accurate estimate of the tithe in their respective areas. In fact, the editors deduce broad trends from the collected statistics and tentatively trace the variations in the tithe over units of decades from the mid-fourteenth century to the Revolution. This analysis permits them to conclude that the often violent fluctuations of the tithe from year to year were caused chiefly by temporary conditions and that the French agricultural system remained essentially static until the mid-eighteenth century, after which there was a moderate expansion.

The editors of the volume, however, readily

recognize the impossibility of estimating total agricultural production from the statistics thus far collected. A few contributors, notably Georges Frêche, argue that such an effort will never succeed because of the nature of the sources and the many imponderables that beset the researcher. Le Roy Ladurie, on the contrary, takes the position that this objective, while unattainable at present, will be achieved after the completion of vast additional research and monographic analysis. The volume therefore represents an interim report with much valuable but incomplete information on the fluctuations of the ecclesiastical tithe and indications of extensive projected research into the agricultural foundations of the French economy.

WILLIAM F. CHURCH
Brown University

SALVO MASTELLONE. *Venalità e machiavellismo in Francia (1572-1610): All'origine della mentalità politica borghese*. ("Il pensiero politico" Biblioteca, 4.) Florence: Casa Editrice Leo S. Olschki. 1972. Pp. 257. L. 4, 500.

Professor Mastellone has further developed the thesis that he first described in the last chapter of *La reggenza di Maria de' Medici* (Messina, 1962) and has now projected it back into the period 1572-1610. Briefly stated, the thesis in its present form holds that between the accession of Henry III and the institution of the *paulette* in 1604 the upper echelon of legally trained French bureaucrats underwent a mental transformation that had its origins in the ideas of Machiavelli and the practice of venality of office. This led them to a new sense of their importance as a separate group at the same time that the French monarchs were finding it necessary to rely on their services to govern the state. Thus the feudal-noble monarchy became the administrative-venal monarchy.

The defense of this thesis places the author in the midst of a steadily growing controversy concerning the nature of society in early modern France. Mastellone has tried to present an answer to one of the central questions of the controversy—the place in society of royal officials. Since his answer is the result of a traditional approach to political theory and is derived from the study of a selection of contemporary controversial writings and secondary sources, it must be regarded as provisional. As such it would best have been presented as a long article. This is especially true since the time lapse necessary for the publication of a book means that several recent articles and

books (notably Myriam Yardeni, *La conscience nationale en France* [1971], and William F. Church, *Richelieu and Reason of State* [1972]) that might have led the author to modify some of his ideas could not be consulted.

Before the thesis of this book can be accepted or rejected a more thorough study of manuscript records is necessary. For example, Mastellone's ideas concerning the Estates-General of 1614 are not substantiated by recent research on the constitution and action of that body. Nor, until more is known, can the development of the process that he describes be said to be limited to the late sixteenth century.

But a warning is in order. Mastellone's intuition led him, when writing the same type of book on the regency of Marie de Médicis, to suggest a number of themes that have been substantiated by subsequent research. In considering the late sixteenth century he has isolated currents of thought that largely escaped both Yardeni and Church. His interpretation of their effect can be ignored only at great risk.

J. MICHAEL HAYDEN
University of Saskatchewan

GÉRARD BOUCHARD. *Le village immobile: Sennely-en-Sologne au XVIII^e siècle*. (Collection Civilisations et mentalités.) [Paris:] Plon. 1972. Pp. 386.

Sennely-en-Sologne, a peasant community of some six hundred inhabitants located south of Orléans, experienced neither the economic nor the demographic expansion characteristic of eighteenth-century France. In Sennely the continual erosion of human life caused by persistently high mortality rates throughout the eighteenth century threatened to destroy the demographic balance. Only the immigration of adults from nearby villages staved off the demographic decline that finally set in during the last decade of the Old Regime.

Sennely was swept along economically by a long-term movement of enclosure for pasturage that reduced the amount of land utilized for the cultivation of grain and undermined the market for rural laborers. Peasant ownership of land was virtually nonexistent, and the little wealth that was generated in Sennely was drained off by absentee landlords, both noble and bourgeois. Fairs and markets declined.

Sennely was disintegrating socially. To the extent that there was social integration this was provided by popular religious traditions. Neither family life, frequently interrupted by the death of a parent, the seigniorial system,

nor village political institutions provided the support necessary for full social existence. Sennely, in short, suffered from acute anomy.

Such, in general, is the "total history" of Sennely. It is unfortunate that this history is not substantiated by adequate documentation. The study rests essentially on eighteenth-century narrative sources, the memoirs of the curés, and the *cahiers de doléances*. Indeed, the main source and the chief guide for interpreting the history of this village is the memoir written by Christopher Sauvageon, curé from 1676 to 1710.

By far the best segment of the book is that which deals with demography, but the author has intentionally limited himself to an investigation of only the broadest outlines of Sennely's demographic history. No attempt was made to reconstruct families; there are no tables for ages at marriage; all intervals between births after the first child are lumped together. Immigration is said to have been essential, yet there are no acceptable statistics on it. The tantalizing discussion of mortality rates indicates that a full demographic history of this village would have been a major contribution. The documentation was available, but it was not used fully.

The section dealing with the agrarian economy and landownership rest on extremely narrow documentary bases. The loss of virtually all the seigneurial records, notably the terriers, is regrettable, but the eighteenth-century fiscal records, the surest source for landownership, were scarcely tapped. There is no demonstration of the advance of large property at the expense of small, no proof that land was in fact passing out of cultivation. Similarly, there are no convincing statistics on social classes and their composition, only rough estimates. We are told that there were landless laborers, small farmers, and artisans, but never how many or how the ranks of these various social groups fluctuated in the course of the century. Social mobility through marriage was supposedly minimal, yet there are no figures on marriage patterns. Crime is said to have been a symptom of social malaise, but the discussion of criminality is anecdotal. Again, there are no statistics on the patterns of rural crime.

With so many excellent French village monographs based on proven methodology as guides, it is difficult to understand how the author could hope to write the "total history" of a peasant village without hard data.

JAMES L. GOLDSMITH
University of Oklahoma

F. V. POTEKIN. *Promyshlennaiia revoliutsiia vo Frantsii* [The Industrial Revolution in France]. Volume 1, *Ot manufakturnykh k fabrike* [From Handicrafts to Factories]. (Akademiia Nauk SSSR, Otdelenie Istarii.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 453.

The Soviet historian Fedor Potemkin has long been active in French history, having published works on the upheavals of 1789 and 1848, as well as studies of labor unrest in the early nineteenth century. The present work is thus the capstone of a half-century's researches in the economic history of France; yet it is not so much a survey of the era 1750-1850 as it is a series of topical studies, arranged roughly in chronological order and preceded by an extensive bibliographical essay.

Throughout the book historiography vies with history for the reader's attention, for it is Potemkin's stated intention to offer a corrective to the numerous Western or "anti-Marxist" studies of this period of French history, marred as they are by the "bourgeois evolutionist" notion that the Industrial Revolution tended to provide eventual solutions to the social problems that it created. The introductory material, some seventy-five pages of historical criticism, shows Potemkin thoroughly familiar with recent Western literature on the subject, though inclined to spar with such earlier works as Toynbee's *Lectures on the Industrial Revolution in England*. There is also a somewhat labored inquiry into the origins of the term "industrial revolution."

Once this preliminary skirmishing is over, Potemkin examines various sectors of the French economy, particularly textile manufacture (wool, linen, cotton, and silk), coal and iron production, and agriculture. The treatment is in each case two-fold—an examination of the industry and a critique of historical literature relating to it. Several of these chapters are particularly impressive, and this is notably the case with the material on the silk industry. The analysis is drawn from a considerable mass of manuscript materials, mostly from the Archives Nationales. Similarly well done are the chapters relating to iron production. Here too, manuscript sources are extensively used; the profile of the industry is complete down to regional nuances. In these final chapters Potemkin is most convincing in his effort to show the linkage between the growth of capitalism and deepening social antagonisms, exploitation, child labor, etc.

There is a certain unevenness about the book, some chapters showing a greater depth of research and perception than others. While many

readers in the West will disagree with some of the author's conclusions, it remains nonetheless a serious and well-documented study.

LEE KENNETT
University of Georgia

JOHN E. N. HEARSEY. *Marie Antoinette*. New York: E. P. Dutton and Company. 1973. Pp. 295. \$7.95.

Marie Antoinette is among the classically tragic figures of history and as such has perennially attracted popularizing biographers. The latest is John Hearsey, an English free lance. Although he provides neither notes nor bibliography, it is evident that he has utilized the principal published sources. His factual errors are few and, in this context, relatively inconsequential. Certain episodes stand out in this account, particularly the affair of the diamond necklace and the queen's imprisonment and trial; over half the book deals with her last four years. Hearsey misses no chance throughout this work to exploit, perhaps a little too insistently, the rich dramatic ironies of the story of this "unwise consort of an incompetent sovereign." Choosing to focus, like so many before him, on Marie Antoinette "simply as a wife and a mother," he has meanwhile narrowly limited his field of view. The wider drama of events in France and Europe is glimpsed only fleetingly, while the secondary characters, outside the royal family itself, remain sketchy, even Count Fersen, the one real love of Marie Antoinette's life. The author contents himself with straightforward narrative, providing little practical motivation or psychological insight. The queen thus appears as essentially the bearer of a destiny, moving inexorably toward its preordained end, which she yet transcends through the growth of her character. To fill in historical background there are appendixes, including glossaries of terms and personalities for the Old Regime and revolutionary period, chronological and genealogical tables, maps, and a brief essay on the causes of the Revolution. Although useful, much of this material could better have been integrated into the text itself. Illustrations are few but well chosen. This work, in short, compares well with others in its genre, for instance, those by Stefan Zweig or Stanley Loomis, though it adds nothing new to the subject, as did André Castelot. It was hardly expected that it give serious attention to Marie Antoinette's considerable political role, studied so long ago by Alfred Arneth, Max Lenz, Jeanne Arnaud-Bouteloup, and Alma Sö-

derhjelm. The definitive biography of both the woman and the queen is still awaited.

H. ARNOLD BARTON
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Carbondale

GERLOF D. HOMAN. *Jean-François Reubell: French Revolutionary, Patriot, and Director (1747-1807)*. The Hague: Martinus Nijhoff. 1971. Pp. viii, 202. 36 gls.

In French Revolutionary scholarship, as Professor Homan suggests, much remains to be done in the area of biographical studies. This is especially true of the lives of the important secondary figures in the Revolutionary drama. Homan's study of Reubell closes just such a biographical gap left open for a surprisingly long time. It also provides a close personal look at the least well-explored episode of the Revolutionary era, the Directory, the period when Reubell reached his eminence.

Homan's articles on Reubell, appearing over the last decade and a half, engendered the hope that they would culminate in a biography of the Alsatian revolutionary. The expectation of a full-scale study of Reubell was heightened by awareness that his life was a fascinating one. Homan's monograph succeeds in maintaining a high pitch of interest in Reubell as a person and as a revolutionary, even though it does not answer all questions raised by Reubell's career. The biography is quite conventional in concept—there are no forays into psychohistory here—and in structure: it progresses in orderly fashion from Reubell's provincial, pre-Revolutionary career through his membership in the Revolutionary national assemblies in Paris, his term as *procureur-général syndic* of the Upper Rhine, to his directorship, the culmination of his life and the most important section of the book.

Reubell was, if anything, an exceedingly "political" man, and accordingly one able to change his stance with relative ease; this may have been the key to his political success, though Homan seems somewhat surprised at every political tergiversation. Homan faces up to the controversies surrounding Reubell's career though he is not always able to resolve them. The connection drawn between Reubell's national politics and his provincial patriotism helps to explain his attitude toward Jews and his special interest in foreign affairs; but that he died relatively poor is not a convincing argument against his possible corruption, in face of his periodic possession of valuable

properties and the sizable retirement grant made to him from secret government funds when he left the Directory.

Reubell's was a fascinating life, one well worth knowing more about. Homan's thorough and extensive research allows him to tell us perhaps all that can be told about Reubell, and he is generally successful in offering answers to an explanation for the issues raised by the director's career. Thus, it is all the greater pity that this biography is so ungracefully written. This may be due, in large part, to poor editing; the book is so full of verbal errors as to suggest that it was not edited at all. Certainly Reubell, and possibly Homan, deserves better.

CHARLES A. LE GUIN
Portland State University

MARC BOULOISEAU. *Bourgeoisie et révolution: Les Du Pont de Nemours (1788-1799)*. (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, 27.) Paris: Bibliothèque Nationale. 1972. Pp. 250.

There is no gainsaying that Marc Bouloiseau, the diligent and distinguished student of the French Revolution, has written a coherent, even an engrossing, narrative of the fortunes of the Du Pont family, specifically of Pierre Samuel, the father, and his two sons, Victor the elder and Irénée the younger.

The data Bouloiseau makes use of lie in the countless private papers, letters, and memoranda of the fabulous Eleutherian Mills History Library that he first began to examine in 1964. As his research developed and deepened, the goal of his investigation became clearer. Here he found a good occasion for "reflection" and an opportunity to restore and reopen "the dialogue of me and others" around the theme of the Revolution. He deals with the repercussions of the great maelstrom on the material life, the opinions, and the attitudes of a family so representative as this "bourgeoisie of talent." He asks in what measure the Revolution engaged or even compromised the future of the Du Ponts. He does not claim to give definitive answers to the queries he raises, but merely to provide the key elements of a broad appreciation.

In the first part, roughly half of the work, he vividly tells the story of the family from the years he calls "The Time of Hope, 1788-1791" up to the resolution of the family in 1799 to abandon France and settle in the

New World overseas. They called this move to America, which they and their descendants were to conquer, the "transplantation." Part 2, entitled "Documents" (private not official papers), backs up the narrative. These documents give the texts that illustrate such developments as the meetings of the Society of Thirty in 1789 and the sporadic peasant "troubles" of 1790. Thus as Bouloiseau tells his story, the emphasis falls not on the subtitle "Les Du Pont de Nemours (1788-1799)" but on the full title "Bourgeoisie et révolution."

What is one to make for the claim that we are here in the thick of the newest of the new history? If the author's words especially in the introductory matter are taken literally, no one but a scholar thoroughly at home with the Revolution itself but also trained in some sociology, social psychology, and "collectivist mentalities" would be qualified to write on the subject. But Bouloiseau's book is not of the avant-garde history. It makes no novel demands of the reader. It is an exceptionally informative and thoughtful study that in fact relates the family to the larger movement and the movement of revolution to the fortunes of a representative liberal bourgeois family. Occasional references to the new vocabulary of the new historians notwithstanding, the volume is not a pioneer essay in collectivist social history. It is written more in the spirit and letter of Georges Lefebvre or Lucien Febre than, say, of Mandrou or Vovelle, Furet or Cobb when the latter deals with collective mentalities. And to think and write in that spirit and manner is in itself no mean achievement.

LEO GERSHOY
New York University

A. V. ADO. *Krest'ianskoe dvizhenie vo Frantsii vo vremia velikoi burzhuaiznoi revoliutsii kontsa XVIII veka* [The Peasant Movement in France at the Time of the Great Bourgeois Revolution at the End of the Eighteenth Century]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1971. Pp. 452.

This volume, a careful study of an immense, complicated subject, is the work of a member of the faculty of Moscow State University, the most prestigious teaching institution in the Soviet Union. It is one of a considerable number of books in the field of modern French history published in the Soviet Union since 1956 or 1958, primarily by men in Moscow, and it reflects the growing interest in French history, especially in radical ideas and move-

ments. On the other hand, the Moscow State University Press published only nine hundred copies of this volume, so scholarly books in the Soviet Union apparently attract even smaller markets than they do in this country.

This study covers only the period from 1789 through 1792, with a fifty-page introduction on the peasants' situation before the Revolution. It concentrates almost entirely upon the countryside, with little reference to the impact of peasant movements upon political developments in Paris or on the international impact of the various upheavals in France. Professor Ado, unlike most Soviet historians, has been allowed to leave his country for research abroad. He has therefore made use of the Archives Nationales, as well as the archives in Allier, Ardèche, Ariège, Gard, Gers, Isère, Yonne, Cher, and Indre. Like all Soviet scholars, in his bibliography he lists Soviet sources first. Thus, he lists Soviet archives before those he used in France, Soviet secondary sources before foreign, and the works of Marx, Engels, and Lenin before those of any other observers or scholars. He relies heavily on Saboul, obviously has a special appreciation for Jaurès, knows well the French scholarship of the Third Republic, and is not well acquainted with English or American publications on the Revolution.

Ado's main thesis is that the Revolution brought benefits to all peasants in demolishing the feudal system, but that the more prosperous peasants were the main beneficiaries, as he believes they were in all revolutions before the November Revolution. The land arrangements made were such that industrialization in France in the nineteenth century was much slower than in England.

The Peasant Movement is much less dogmatic than most Soviet scholarship, perhaps because Ado's extensive work in French archives has persuaded him that he should study all materials before reaching conclusions. He even suggests that additional research needs to be done on this subject before definite conclusions can be reached concerning the peasants' goals and role.

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C. STEWART GILLMOR. *Coulomb and the Evolution of Physics and Engineering in Eighteenth-Century France*. Princeton: Princeton University Press. 1971. Pp. xvii, 328. \$13.50.

The name coulomb was given to a quantitative unit of electricity long after Charles Augustin Coulomb (1736-1806) had made an early estimation of the efficiency of laboring men and later determined the force laws of electrostatics and magnetism. After a career as engineer and physicist and public service in the years before and after 1789, he died at a moment when individuals were apt to be quickly forgotten and thus neglected by later historians of science. In the absence of any full study of the man, Professor Gillmor's well-documented book makes much information available concerning Coulomb's work and times.

Two chapters of this revised dissertation present biographical details, while four more offer a technical survey of problems ranging from the theory of structural stresses and design to soil mechanics and revetments, leading in turn to activity in the context of the Académie des sciences, torsion balances, and the quantitative investigation of static electricity and magnetism.

Coulomb's contribution to the general history of science is characterized by Gillmor's interesting claim that Coulomb's studies led to the final rejection of Cartesian vortexes and effluvia as explanations of magnetism and light and to the general acceptance of Newtonian mechanics as the basis for emerging branches of physics. Elsewhere the author suggests that the rise of the modern mode of this science grew from Coulomb's fusion of the curiosity of the natural philosopher with the engineer's contact with reality and the harmony of rational analysis.

Service on public commissions marked Coulomb in later years as a social engineer. He worked on the design and planning of canals for upper Brittany, the improvement of water supply and hospitals in Paris, and a report (1776) for the reorganization of the *Corps du génie*, of which he had been a member for thirty years. In this document, reproduced on pages 255-61, one finds that Coulomb's thinking about the value of useful work recalls that of Voltaire, who desired to see manpower usefully employed, and, in particular, that enrolled in monastic orders. Apart from this one text, the reader finds little contact with the ideological trends of the century. Coulomb lived a quiet life politically, apparently unconcerned about the state of France; he stayed away from Paris during the Terror except for a brief visit for the funeral of Lavoisier, and he served the newly formed Institut under the Directoire and Bonaparte. He remains a shadowy figure about

whom Gillmor could find few personal details. From the *inventaire après décès* the author re-creates the modest milieu of the apartment near the Institut—a poor substitute for the characterization that might have been drawn from analysis of such highly personal documents as the report on the reorganization of the *Corps du génie*.

This fully researched book is perhaps a product of the comprehensive survey of the whole field of the history of science associated with the editing of the *Dictionary of Scientific Biography* now well advanced in production.

HARCOURT BROWN
Parry Sound, Ontario

RENÉ PICHELOUP. *Les ecclésiastiques français émigrés ou déportés dans l'État Pontifical, 1792-1800*. (Publications de l'Université de Toulouse—Le Mirail, Series A, number 15.) Toulouse: Association des Publications de l'Université de Toulouse—Le Mirail. [1972.] Pp. 302. 51 fr.

This book opens with a preface by Jacques Godechot, who explains that general studies have given the impression that French emigré priests were better treated in Protestant countries than in Catholic states where they were regarded as potential carriers of revolutionary ideas. This study offers confirmation for the Papal States. It is based almost exclusively on manuscripts found in the Vatican Archives.

The organization is effective. Abbé Picheloup examines successively the arrival of emigré and deported priests between 1791 and 1794, the conditions of their existence while there, and their return to France between 1795 and 1800. His research has revealed that only about 200 priests arrived by the end of September 1792. Following the expulsion order of August 26, 1792, this trickle turned into a flood, and by the end of 1792 there were about 2,200. Only 300, however, were allowed at Rome. The remainder were scattered in religious houses throughout the papal territories where superiors, guided by rules laid down by Monseigneur Caleppi, provided supervision. After a tapering off in 1793, another wave came in 1794 before the French advance in that year. As a result there was a total of 3,000 emigrants by the end of 1794. Caleppi refused to take any more because of popular discontent caused by a rise in prices that accompanied the emigration.

Those who were allowed in had to demonstrate their religious and political orthodoxy and adjust to the life style of the house that

sheltered them; they were forbidden to leave the diocese without permission and found that they had little to occupy their time except for the saying of occasional masses. This discussion of their daily life suffers from a dearth of source material from French priests except for the fragmentary evidence that happened to find its way into the Vatican Archives. And Picheloup fails to describe the activities of Cardinal de Bernis or Cardinal Maury.

In conclusion, though the evidence is sketchy, Picheloup says that, while the emigrés accepted Napoleon, most of them probably still awaited the restoration of the monarchy and, as a result of their dependence on the pope during this period, took their first steps in the direction of ultramontanist. In an appendix the author offers a series of documents and provides lists of those present in the Papal States and at Rome. The lists, however, appear incomplete and, because of their arrangement, are difficult to use. There is no index.

LYNN OSEN
Beloit College

G. GERBAUD *et al.* *La Révolution dans le Puy-de-Dôme*. Foreword by A. SOBOUL. (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, 26.) Paris: Bibliothèque Nationale. 1972. Pp. 320.

This is a fine collective work in French Revolutionary regional (Puy-de-Dôme) economic and social history—the fruit of research in the department and municipal archives by four students of the Faculté des lettres of Clermont-Ferrand. As Albert Soboul indicates in the preface, too often in the past French Revolutionary history has concentrated on the stirring events in Paris—the hub of the Revolution, true, but then again significant economic and social events in the provinces, later departments, would have a profound impact on the capital. In fact, capital and regional history are closely interrelated. Within the last forty years authorities such as Georges Lefebvre and Albert Soboul among other well-known scholars have used and have encouraged others to investigate valuable historical sources found in regional archives. There is much yet to be done, however.

Gérard Gerbaud in the first piece in the collective work, "Esprit public et police générale dans le district of Clermont-Ferrand (juin 1793-été 1794)," gives a vivid portrayal of the politically mixed bourgeois leadership categories, whether Girondin or Jacobin, and their frictions and sometimes cooperation in the face

of general peasant and worker opposition to such measures as the raising of troops, requisitioning of grain, fixing of prices, and the enforcement of anticlerical legislation. By the summer of 1794 local Revolutionary administrations were discredited, and the sans-culotterie regarded the Ninth Thermidor and its consequences with relief.

Jean Pètelet in the second article, "La Vente des Biens du Clergé dans le District de Clermont-Ferrand (1790-1804)," which was reinforced by valuable statistical tables (pp. 111-34), concludes that although the sale of clerical lands brought very limited revenue to the state, because of undervaluation of lands and inflation of Revolutionary currency, and did not relieve the misery of poor peasants, it redounded in favor of the bourgeois (merchants and lawyers largely) who got 71.4 per cent of the lands and 61.4 per cent of the buildings of the clergy in the district. Indeed, many peasants would simply change masters.

Daniel Martin in the third article, "La Vente des biens des Émigrés dans le District de Clermont-Ferrand (1792-1830)," reviews for a longer period the disposition of emigré property in the district. His information is backed by an excellent series of statistical tables and graphs (pp. 218-61). Once again the peasants would profit but modestly from the sales. They acquired twenty-seven per cent of the landed property while they represented forty two per cent of the purchasers. On the other hand, amazingly, members of emigré families, generally represented by agents, were able to purchase eventually more than twenty-six per cent of the lands offered for sale. The bourgeois category (merchants, lawyers, and *propriétaires*) would get the lion's share—forty per cent.

The last article in the collection, "Les Fêtes civiques dans le Département du Puy-de-Dôme sous la Révolution" by Annie Lamadon, is an interesting vignette of one aspect of French Revolutionary social history—civic festivals. Despite the patriotic ardor of Revolutionary leaders, whether deists, agnostics, or atheists, who wanted to replace traditional festivals, such as Catholic feast days, dynastic celebrations, and baladoires (games and dancing) with new patriotic holidays (July 14, August 10) or festivals dedicated to Robespierre's Supreme Being, the mass of people in the department would retain their preferences for the traditional festivities (excepting the dynastic). It was the bourgeois, the class that benefited the most from Revolutionary change in the area, that proved most flexible in adapting to the direc-

tives emanating from Paris relating to civic festivals.

To conclude, this collective study, a result of solid research in regional archives, is a significant contribution to French Revolutionary regional economic and social history and is recommended to all serious students who want to know more about events outside of Paris during the French Revolution.

JAMES MAXWELL MOORE

Norwich University, Northfield

WILLIAM SCOTT. *Terror and Repression in Revolutionary Marseilles*. New York: Barnes and Noble. 1973. Pp. xii, 385. \$23.75.

The core of Professor Scott's book concerns the Revolutionary Tribunal of Marseille, the judicial body that administered the Terror in that city following the federalist revolt of 1793. He makes extensive use of the verbatim proceedings of the tribunal to illumine the attitudes and ideologies of the judges and, to a lesser extent, of the judged. His study of the tribunal is preceded by a narrative of the Revolution in Marseille from the calling of the Estates-General to the federalist revolt. It is a puzzling story: always on the extreme left in the early years of the Revolution, Marseille suddenly veered away from the radicalism of Paris in April and May of 1793. The revolt, interestingly enough, was led by the sections, the same bodies used by the sans-culottes to impose their radical policies on the Convention. How and why the sections adopted a moderate line remains something of a mystery in Scott's narrative. There was no significant turnover of personnel that might have caused such a change, no dramatic event propelling erstwhile revolutionaries into reaction. Indeed, in the beginning the events that became the federalist revolt must have seemed to most *Marseillais* much like earlier episodes in the Revolution, in which the *Marseillais* had always showed a willingness to take action on their own, independent of any direction from the capital. Always an enigma, the federalist revolt remains so in Scott's account.

Nor is the enigma clarified in his chapters on the Revolutionary Tribunal. In his analysis of the tribunal's proceedings Scott has other questions in mind: What kinds of offenses did the tribunal consider most serious? Were the tribunal's decisions based on adequate evidence? Were suspects allowed to defend themselves? In other words, his analysis is meant to inform us about the court and its officers, not about the federalists. His conclusions are interesting and

solidly documented. The tribunal was harsh but judicious. It sentenced nearly three hundred to death, but its judgments were based on a serious effort to establish guilt. No one was condemned without adequate documentary evidence, and defendants were permitted to argue their cases at some length. The tribunal was harsher in its judgments of rich and well-educated defendants than of poor and ignorant ones; peasants, artisans, and workers were often forgiven their participation in the federalist movement on the grounds that they had been misled. The death penalty was imposed above all on those who had acted as officials of the sectionary regime; serving in the federalist army or national guard or merely displaying federalist sentiments earned lesser penalties and were sometimes forgiven altogether. In short, the Terror in Marseille showed none of the excesses evident in Lyon, Nantes, or Toulon.

This book is a competent and useful piece of scholarship that adds to our knowledge of the way the Terror operated in the provinces. But it fails to illuminate the central question of the Revolutionary history of Marseille—How did the city whose volunteers helped topple the monarchy on August 10, 1792, and whose name adorns the greatest anthem of the Revolution, turn to counterrevolution in 1793? The federalist revolt of Marseille still awaits its historian.

WILLIAM H. SEWELL, JR.
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ALF ANDREW HEGGOY. *The African Policies of Gabriel Hanotaux, 1894–1898*. Athens: University of Georgia Press. 1972. Pp. viii, 161. \$7.50.

A historian writing about a historian who became a statesman presents an intriguing situation. Dr. Heggoy, of the University of Georgia, deals with the prolific historian and archivist, Gabriel Hanotaux, who was the French foreign minister from May 30, 1894, to November 1, 1895, and from April 29, 1896, to June 15, 1898. Affirming that Hanotaux, Théophile Delcassé, and Étienne were offspring of the imperialistic school of Ferry, de Freycinet, and Gambetta, the author's theme is that Hanotaux's motive was the attainment of prestige for France, economic considerations being of no great significance. Carefully balancing his policies, Hanotaux specifically sought the consolidation of all French territories in Africa. Dr. Heggoy states that Hanotaux's method was pragmatic, that is, first creating a problem and then offering to negotiate it and any other outstanding issue. Hanotaux approached each question with a

mastery of details—geography, history, and treaties.

He emerges with high marks. In Morocco his introduction of a vice-consul and maintenance of the status quo made possible the eventual French takeover. In the Congo Hanotaux persuaded Leopold II to allow French access to the Upper Nile. Although preferring to keep her as a protectorate, Hanotaux annexed Madagascar. In Tunisia he negotiated the cancellation of twelve conventions with European powers and kept Tunisia as a protectorate. In West Africa he secured Nikki and a large area east of Say on the Niger's left bank. Except for French Somaliland Hanotaux consolidated all French holdings in Africa. His failure was the Egyptian question. Here he is faulted because his customary moderation gave way to challenging Britain, which enjoyed naval superiority.

Unhappily Dr. Heggoy's notes are relegated to the rear where they are closely clustered together on fourteen pages. There is a wide range of sources. Missing are Stengers's *Revue Belge* articles on the Marchand expedition and John Flint's *Sir George Goldie and the Making of Nigeria* (1960).

The map of French Africa in 1894 does not pinpoint the French possessions. Ras Makonnen was never a king in Ethiopia, which, mistakenly, is repeatedly called Abyssinia.

For the general public the author presents a convenient summary of a remarkable man's achievements. A moderate Republican, Hanotaux promoted the *mission civilisatrice* that, for him, entailed consultation with cabinet colleagues and courteous negotiations with European powers. He was a man of his time in not considering the right of Africans to manage their own affairs. Hanotaux's love was France, the country that, sadly, though he lived to 1944, never recalled him for a third term at the Foreign Ministry.

ERVING E. BEAUREGARD
University of Dayton

DOMINICK LACAPRA. *Emile Durkheim: Sociologist and Philosopher*. Ithaca: Cornell University Press. 1972. Pp. x, 315. \$12.50.

Emile Durkheim has been getting a lot of attention of late. Dominick LaCapra offers an explication of Durkheim's thought as it evolved in his major works. This is a hard task. By the time Durkheim came along, lucidity had gone out of fashion in French writing on man and society. His work, highly abstract, vague, and ambiguous, has invited intense controversy.

LaCapra regards Durkheim as as much a moral philosopher as a modern social scientist. The author ascribes Durkheim's ambivalent cast of mind to his position as a transitional figure. A thoroughgoing rationalist—"a Cartesian neo-Kantian" in his early years, when he published *On the Division of Social Labor* (1893)—Durkheim came increasingly to feel that this position was inadequate, both as a tool of social analysis and as the basis of prescriptive recommendations for reform. In a later period, without abandoning his philosophical stance, he gave freer rein to a dialectical bent and to the preoccupation with religion that culminated in *The Elementary Forms of the Religious Life* (1915).

Like Marx, Durkheim sought to make sense of the rapid transformation of his own society. LaCapra insists on the importance of understanding Durkheim's distinction, fundamental to all his work, between social normality and pathology. It is unfortunate that these are among the most abstract concepts in Durkheim's vocabulary, and he never got around to applying them to any historical societies. Nevertheless, he clearly regarded his own society as pathological, "beset with varying sorts of internal contradictions." A state of social normality, on the other hand, "was characterized by a highly specific sort of functional integration."

This book belongs to the tradition of intellectual history that concerns itself almost exclusively with the explication of formal systems of thought. LaCapra, his attention riveted on Durkheim the thinker, has little to say about the milieu in which Durkheim lived and worked. But Durkheim was in some respects the perfect sociologist of the Third Republic—in his sympathy for a reformist socialism and his hostility toward revolutionary syndicalism, in his intense patriotism and his preoccupation with order—a pillar of the republican academic establishment as well as a major social theorist. Steven Lukes's monumental *Emile Durkheim: His Life and Work, a Historical and Critical Study* (1973) restores Durkheim and his circle to prewar France.

JOHN E. TALBOTT
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SUZANNE BERGER. *Peasants against Politics: Rural Organization in Brittany, 1911-1967*. (Written under the auspices of the Center for International Affairs, Harvard University.) Cambridge,

Mass.: Harvard University Press. 1972. Pp. xi, 298. \$11.75.

The author writes that "the modern European state has lived upon a reservoir of soldiers and electors provided by the peasantry, but the peasants have remained the object of politics and not its master." With the modern political organization of the countryside, however, peasant protest assumed two contrasting forms. One was the corporative movement that sought to regulate problems beyond the state; the other was political formations that transmitted peasant grievances to the state. The present study concerns rural corporatives in Finistère, a department of conservative traditions and struggling family farms at the dead end of Brittany. Its most interesting chapters concern the conservative origins of the movement near the beginning of the present century when rural nobles set its foundations in social catholicism: "To tighten the ties which unite the great rural family, to make our associations a vast school of professional solidarity, of *la paix sociale*, and of Christian charity"—in a word, to advance rural life without disrupting its traditional social relationships. The road led from agricultural syndicalism to professional corporatism, but the results were the same: the elites led, the peasants paid dues, and Finistère remained the same. After 1958, however, a surge of grass-roots activity and direct action suddenly modernized Finistère agriculture and opened a way toward the larger community of France. But the dynamic advance of some farms left others behind, and the resulting social conflicts caused a retreat into the conservative fortress of the "great rural family." New tractors did not change the old politics.

Such are the outlines of this competent work in political science, based on personal interviews and research in departmental and syndical archives. At times Berger attempts to widen the significance of her research by inquiring into the associational behavior of peasant organizations in neighboring areas. Her remarks on left-wing syndicalism are sketchy, but her statistical measurements of the more advanced state of political opinion in Côtes-du-Nord, an adjacent department where peasant protest assumed party-political form, support her conclusion that corporatives obstructed the integration of Finistère into the national political system. The powerlessness of the peasantry, she contends, "reflected more or less deliberate decisions by the political elites, but the policies succeeded only because those organizations that

the peasants built themselves did not challenge—in fact, supported—the exclusion of the peasants from full political participation.” In this sense *Peasants against Politics* is a study of politics against peasants.

GILBERT ALLARDYCE
University of New Brunswick

JEAN-JACQUES BECKER. *Le carnet B: Les pouvoirs publics et l'antimilitarisme avant la guerre de 1914*. (Publications de l'Université de Paris-Nanterre. Series A: Thèse et travaux, number 19.) Paris: Éditions Klincksieck. 1973. Pp. 226. 48 fr.

The incident of the Carnet B is a historical event that in a sense never occurred. For decades before 1914 the French government kept a list of suspects to be arrested in case of war. Although the original purpose was to identify spies, by 1909 the list was an elaborate system focused upon labor revolutionaries who might sabotage mobilization. In August 1914, however, the interior minister decided not to invoke the Carnet B, correctly predicting that French labor would prove loyal in the crisis.

Some years ago Jean-Jacques Becker published an interesting if unsurprising paper on this famous list of *saboteurs*. In response to his letters of inquiry, only twenty-nine out of eighty-seven departmental archives could locate papers relating to their local carnets, the other dossiers having been destroyed or never released by the government. The spotty nature of these sources (only forty per cent of the French nationals on the list could be identified, and some industrial areas were not represented) prompted Becker to write an impressionistic description of the more complete departmental records. He found great variation between areas as to the criteria and the care used in compiling the local carnets. The implicated men were known, in general, for their revolutionary syndicalism, anarchism, or Hervé socialism, and the essential criterion for inscription in the carnet was not planning but merely advocating the obstruction of French mobilization.

This present work is an expansion of the previous research to include the more general question of French governmental reaction to labor antimilitarism before 1914. The result is a disjointed union between Mr. Becker's study of the Carnet B and a rapid survey of better-known events, a survey that serves to place the carnet in context with other events. The author stresses, for instance, the great expansion of the carnet to include syndicalist revolutionaries at

a time when organized labor was constantly clashing with the Clemenceau and Briand cabinets. Immediately before the war, moreover, many syndicalists were removed from the list as harmless, a change that reflected declining radical labor activity. It is unfortunate that Becker's summary of events is so broad that it neither introduces the general reader to the subject (why, for example, does the author fail to note the French Labor Confederation's prominent efforts to make antimilitarism part of the international labor program when such efforts were undoubtedly related to official actions against the Confederation?) nor balances the specialized nature of the Carnet B analysis.

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EVELIO VERDERA Y TUELLS, edited and with an introduction by. *El Cardenal Albornoz y el Colegio de España*. In three volumes. (Studia Albornotiana, 11–13.) Bologna: Real Colegio de España. 1972; 1972; 1973. Pp. 726; 742; 708.

Sometimes one wonders what might have been if, at a certain point, history had taken another turn. If the papacy had remained in Avignon, for instance, where the supreme pontiffs resided since 1305, how would Europe have developed? What of the future relation of powers between France, Spain, Italy, the Empire and the rest of the Continent? Would Luther have found the Holy See at Avignon as offensive to him as that of Rome? Perhaps the Reformation, Counter Reformation, and innumerable other conflicts, spiritual and military, might never have occurred.

That it came otherwise, that the popes were able to return from their “Babylonian exile” to Rome, is generally accredited to the efforts of Cardinal Gil Álvarez de Albornoz (about 1300–67). A consummate diplomat, military strategist, and legislator, Albornoz, a native from Cuenca, under Alfonso XI had been archbishop of Toledo from 1339 until he exiled himself in 1350 from Castile on the accession of Pedro I, the “Cruel.” In 1353, when Innocent VI sent Albornoz as his legate to Italy, giving him full powers to bring the lost ecclesiastical territories back under the authority of the Church, nothing was left of the papal domains except two tiny townships, Montefiascone near Viterbo and Montefalco, in the duchy of Spoleto. The rest had splintered into particularist anarchy. In Rome Cola di Rienzo, the People's Tribune, was still in power. Other cities and principal-

ties in Umbria, the Marches, and the Romagna were held by individual "tyrants," such as the Malatesta and the Visconti. "Single-handed" seems a fitting attribute for the cardinal's achievements: he succeeded on his own in raising the necessary finances and a capable military force. Having taken part earlier in the battle of Salado against the Moors (1340), Albornoz was used, like many Spanish clerics of the *reconquista*, to fight—figuratively speaking—with the lance in one hand, the crucifix in the other. A master especially of siege warfare (the indispensable method of bringing down those dozens of walled townships and castles), he managed to terrorize central Italy into submission. As the former patrimony of Saint Peter was being restored, Albornoz also wrote a new constitution for the Church of Rome. Often the cardinal of Spain acted, if not holier, more in the popes' interest than the popes themselves. Innocent VI and his successor Urban V, Frenchmen both, were not always in accord with the Castilian's efforts in their behalf. Near the end of his second sojourn in Italy, where he spent thirteen years in all, Albornoz was able to persuade Urban to come to Italy for his triumphant entry into Rome. But the cardinal died on the way, in Viterbo, before he could set foot in the place of his vision.

So, two hundred years before the Spain of Philip II would perfect its domination of disintegrated Italy, this exiled Castilian had already exerted perhaps the most decisive Hispanic influence on that country. Still, in view of the fact that Urban's temporary residence at Rome was followed by rekindled warfare about the Papal States and, thereafter, by the Great Schism, Albornoz's accomplishments might have been undervalued and even forgotten had he not set to himself a lasting monument by founding in 1366 the Spanish College at Bologna.

Seven years ago the college honored the sixth centenary of Cardinal Albornoz's death. The memorial events extended into 1969 with an exhibition of books and a congress of studies of the cardinal. The rector of the college, Evelio Verdera y Tuells, who directs a series of *Studia Albornotiana* of which these three massive volumes are numbers 11–13, has assembled here various speeches from that congress and augmented the collection by other recent contributions, a total of seventy-five, all dedicated to Albornoz and to the college. Despite this link, the studies by the long international list of writers deal with the most divergent matters and range in time from the twelfth century to

the present. In the opening article, for example, Salvador de Moxó describes the rise of the house of Albornoz from minor provincial nobility to first-line aristocracy under the Trastámaras by the end of the fourteenth century; the following studies of volume 1 examine in a roughly chronological order all possible aspects of Albornoz's activities in Italy, often based on new documentation. Much of volume 2 deals with the college, including biographical studies of some sixteenth-century alumni, such as the doctor Pedro Carnicer, one of the physicians of Emperor Ferdinand I, discussed by Marcel Bataillon who reflects on the Spanish medical profession of that time as pre-empted by New Christian descendants of Jews; Joseph R. Jones sketches one of Emperor Charles V's councillors, the doctor Hernando de Guevara. Volume 3 has about a dozen papers on the wide scope and influence of Albornoz's contributions to canon law; it contains also the first book-length installment of a major study by Antonio Domingues de Sousa Costa about the Portuguese at the college in the fifteenth century and ends with biographical sketches of some outstanding Spaniards, twentieth-century alumni. The articles, all of impeccable scholarship, should be of great interest not only to historians of Catholicism but also to aficionados of Iberian and Italian political and cultural history.

ERIKA SPIVAKOVSKY

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N. CAULIER-MATHY. *La modernisation des charbonnages liégeois pendant la première moitié du XIX^e siècle: Techniques d'exploitation.* (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, number 192.) Paris: Société d'Édition "Les Belles Lettres." 1971. Pp. 308.

"The study of the modernization of the coal mines of Liège in the course of the first half of the nineteenth century has shed light upon the important phenomenon of the industrialization of a region which has played a capital role in the economic growth of Belgium and it has permitted the specification of several ideas often too vaguely raised in the explanation of that fundamental social transformation which one is still in the habit of calling the Industrial Revolution." Thus, in a sentence characteristic of his prose, Professor Caulier-Mathy states the justification for his book. Readers who would consult it for information regarding "modernization" and the "Industrial Revolution" should

be warned, however, that the author conceives of these notions exclusively in terms of technological innovations. His study begins in the early eighteenth century and follows Liégeois coal mining through the Habsburg, French, Dutch, and Belgian regimes up to 1850. Caulier-Mathy catalogs even the most incidental developments in coal mining technology in abundant, and sometimes excruciating, detail. He supplements his text with twenty-one illustrations, five graphs, and eleven statistical appendices. There is even a glossary, so that if you have been bothered by what a *bouxlay* is, or a *pahage*, or even a *tocque-feu*, your troubles are over. As Lucien Febvre once objected of a book under review, "Est l'homme dans tout cela?" Caulier-Mathy lays great stress on the role of both engineers and entrepreneurs in initiating the various innovations, but in his account they are only engineers and entrepreneurs, not people. The miners themselves invented nothing of value, and thus they appear largely as the collective breathers of gas and raisers of the annual yield. It may be useful to learn, for example, that the Mueseler lamp replaced the occasionally explosive Davy lamp in 1838, or that Lefebvre d'Hellancourt was probably the real author of the ministerial circular of 18 Messidor of the Year IX. But until we relate such "facts" to human experience, we are not going to learn much more about such subjects as "modernization" and the "Industrial Revolution."

GEORGE FASEL
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Columbia

OLE FELDBÆK. *Dansk neutralitetspolitik under krigen 1778-1783: Studier i regeringens prioritering af politiske og økonomiske interesser* [Denmark's Neutrality Policy during the War of 1778-1783: Studies in the Government's Assessment of Political and Economic Priorities]. (Københavns Universitet, Institut for Økonomisk Historie, Publication number 2.) Copenhagen: G. E. C. Gads Forlag. 1971. Pp. 212. 34.50 D. kr.

The American War of Independence, in which France, Spain, and Holland were also involved, affected in no small measure many of the minor European powers. One of these was the dual monarchy of Denmark-Norway, which during the previous century had developed into a maritime nation of some importance and was also an imperial nation of sorts with far-flung colonies in Greenland, the West Indies, West Africa, and India. Like most trading nations

without an actual stake in the conflict, Denmark-Norway desired to stay neutral in the war that commenced between England and France in 1778.

Denmark was to some extent greatly hampered in its wish to reap the benefits that fall to a neutral nation by the fact that it had entered into an alliance with Russia in 1773; Danish maneuverability was indeed greatly restricted, but through the efforts of able statesmen it may be said that the country weathered this particular storm with flying colors.

The so-called League of Armed Neutrality was formed in 1780, and it proved a rather successful undertaking. Denmark, for one, experienced an economic boom from 1780 to 1783, with the shipping and trade industries flourishing. Although the country suffered in many ways and went through a difficult time, the government managed to exploit to the fullest extent the alternatives that flow from a neutral status.

The nation's relations with the outside world were guided, then as now, by three overriding considerations—political, economic, and those relating to security. How the Danish government succeeded in putting behind it what may be likened to an obstacle course, how it weighed the economically desirable against the politically possible, and how it assigned priorities to the various conflicting interests are well brought out in this interesting case study.

The author, who is an associate professor of history at the University of Copenhagen, has produced not only a very readable volume but also a fine contribution to the study of this period in Danish history.

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American-Scandinavian Foundation

JOHNNY LEISNER *et al.*, editors. *Festschrift til Poul Bagge på halvfjerdsårsdagen, 30. November 1972: Fra en kreds af elever* [A Festschrift for Poul Bagge on His Seventieth Birthday, 30 November 1972: From a Group of His Pupils]. Copenhagen: Danske Historiske Forening. 1972. Pp. 400. 95 D. kr.

A miscellaneous collection of articles with an appended bibliography reveals something of both the person honored and its contributors. The bibliographical listing at the end provides a complete view of Bagge's work, which runs mainly to editing—with Aage Friis, documentary collections on Schleswig-Holstein; the *Excerpta Historica Nordica*; the *Historisk tidsskrift* from 1943 to 1966; and others—and the

writing of a large number of reviews and necrological notices. Bagge also liked involvement in historical and current affairs through writing long articles for both local and Copenhagen newspapers. Povl Bagge's warmth of personality, close comradeship with colleagues and students, and his dominant position in the Danish historical profession are indirectly evident, as is also a careful methodology. At the same time the narrowness of Danish seminar methods crops up in the minute, precise articles on small matters.

Predominant in the *festskrift* are studies of historians and historiography, as in an examination of Erasmus, the theory of kingship and Christian II in the *Skibbykrønike*, or Niels Pedersen Slinge's purported fabrication of a letter concerning an agreement between burghers and king on royal absolutism. Then, too, there are critical treatments of Ludvig Holberg, Gerhard Ritter, Edvard Holm, and Jens Schelderup Sneedorff that provide insights into concepts of the "good emperor," German nationalism and class concepts, politics and diplomacy of eighteenth-century Denmark, and the Danish intelligentsia of the same period. More detailed and typical are studies of voting behavior in the plebiscite on the sale of the Danish West Indies in 1916, negotiations for laying telegraph cables in the Far East, Danish neutrality in 1853, or payment of parliamentary members, "one item on which members of parliaments can agree."

To make sense of this dispersion of subjects from the sixteenth to the twentieth centuries is obviously impossible. Some reference should be made to the excellent interpretation of Gerhard Ritter's philosophy and writing or to the several articles touching on social classes and their relation to politics and religion. An article on slavery and religion in the eighteenth century is, however, both an attack on an unrelated thesis of the contemporary religious leader and writer, Hal Koch ("better western Christian capitalism than East European Communism"), and on the hypocritical attitudes of eighteenth-century clerics. The grotesque aspects of this subject become gross with careful selection of evidence, even if it reveals interests of clergy in converting African slaves.

Where authors get down to "normal" subjects, it is a detailed narrative of documentary evidence. Complications of mergers of cooperative and private slaughteries in the 1890s run out in thin air since the conclusion concludes nothing or the diplomacy of the Crimean War and Danish neutrality eventually mean nothing

either in that war or European politics. They are of some use for Danish history and offer a bit of insight into international matters of more concern to historians of other cultures. The *festskrift* is an addition to Danish historical memorabilia and to commendation of and celebration for Povl Bagge's seventieth birthday. But its contribution to either European or Danish historical study is marginal both because of topic, language, and content.

As a footnote, it is interesting to eye the first item in Bagge's bibliography: a review of Lawrence D. Steefel's work of 1932 on Schleswig-Holstein. It and other contributions reflect Bagge's interest and knowledge of non-Danish scholars and in particular his many friends among Americans.

RAYMOND E. LINDGREN
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OTTO STEIGER. *Studien zur Entstehung der Neuen Wirtschaftslehre in Schweden: Eine Anti-Kritik*. (Wirtschaftswissenschaftliche Abhandlungen: Volks- und betriebswirtschaftliche Schriftenreihe der Wirtschafts- und Sozialwissenschaftlichen Fakultät der Freien Universität Berlin, number 28.) Berlin: Duncker & Humblot. 1971. Pp. 235. DM 68.60.

Steiger's book addresses itself primarily to the economist. It contends that the new economics (*Neue Wirtschaftslehre*) does not initially derive from Keynes's *The General Theory of Employment, Interest and Money* (1936) but that from 1908 on, the Stockholm School (Knut Wicksell as a precursor and then M. B. Hamilton, Erik Lindahl, Gunnar Myrdal, Bertil Ohlin, and Ernst Wigforss) worked out the principles of the theory. Stress is also put on the English predecessors of Keynes: the Fabians, Beatrice Webb ("The Minority Report of the Poor Law Commission"), G. H. D. Cole, and others. While the theory and its mathematical formulations, particularly with regard to money and prices, are of but marginal interest to the historian, two aspects concern him directly: the author's contribution to the history of economic thought and the part dealing with the influence of the Stockholm School on Swedish politics, especially among the Social Democratic party, between 1908 and 1930.

In the face of depression and unemployment, which constitute the focus of the book and the chief concrete economic issues of the time, the Stockholm School sought explanations for the economic factors underlying the recurrent ec-

onomic crises resulting from oversupply, unemployment, and patterns of savings activities. It maintained that in times of stagnating private industry, it was the duty of the state not to hoard money, balance the budget, and postpone expenditures but to spend, plan useful work projects, and thereby accelerate economic activities (p. 79). This view challenged not only laissez-faire concepts but also the progressive policy of supporting the unemployed by *Notstandsarbeiten* (though at minimum pay) or by unemployment insurance. It also challenged many postulates of Marxism.

His polemics against Keynes's primacy lead the author to emphasize that when the Socialists came to power in Sweden at the beginning of the Great Depression they harked back with their projects not to Keynes but to the earlier teachings of the Stockholm School—teachings that, to be sure, represented ideas then developing in various places.

The author's concise and competent presentation will stimulate the historian to comparisons with labor policies in Bismarck's time, with German experiences under Brüning, American experiences under F. D. Roosevelt, and programs in Russia, England, and elsewhere. With its accent on unemployment and depression it will also show the difference against recent developments with opposed trends: inflation, devaluations, population increases, new international relationships, and changed attitudes of labor parties and organizations. The historian will do well altogether to occupy himself with the specific issues raised in the book and will greatly profit from it when tracing the historical process underlying the evolution of economic thought.

WALTHER KIRCHNER
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PATRICK RILEY, translated and edited with an introduction and notes by. *The Political Writings of Leibniz*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1972. Pp. viii, 206. \$13.00.

That something has not yet happened hardly provides a sufficient reason that it ought to happen. As Leibniz might have said, one can imagine an infinite number of books not compossible, given the inherent limitations of the enterprise, with the best of all scholarly worlds. In the preface to what purports to be a representative selection from Leibniz's political writings, Patrick Riley commends his publisher

for "willingness to revive interesting and unaccountably neglected" material. Although the slender volume is certainly respectable, neither Riley's lengthy introduction nor the relatively few pages of text hitherto unavailable in English are quite enough to inspire complete confidence in his publisher's judgment.

Riley is quite right, of course, that no one will pretend that Leibniz's political writings rival those of his great contemporaries, Hobbes, Spinoza, and Locke. Where political thought is concerned, be it admitted, Leibniz was apparently fully as conventional and prosaic as he was innovative and prescient in mathematics, logic, and dynamics. The question, alas, is not whether the variegated assortment of letters, manuscript fragments, casual remarks, and commissioned exercises assembled here are "worthy of some attention," but rather whether their merit derives exclusively from the fact that they are Leibniz's. If indeed Leibniz's political thought has hitherto suffered neglect, in light of the power of his philosophical ruminations in other areas, one may hardly consider such neglect "unaccountable." Had Leibniz actually produced the "political system" with which he is credited, but which fails to materialize clearly either in Riley's introduction or in the assembled documents, such neglect would remain eminently accountable, if not quite as unexceptionable as otherwise is the case.

It seems only fair to add, however, that had a political system emerged out of the rather amorphous farrage evidenced in this collection, it would no doubt have done so as an integral feature of Leibniz's metaphysical synthesis. Nor is there any reason to quarrel with the attempt to read any aspect of Leibniz's thought programmatically, in terms of his quest for a true *philosophia perennis*. For, indeed, Leibniz envisioned a universal architectonic structure with God at the vertex and a perfect commonwealth of independently orchestrated, yet concordant voices expressing his glory in ordered sequence throughout. If this is what Riley means when he speaks of Leibniz's "rationalized medieval system," then, doubtlessly, Leibniz's political utterances (such as they were) should be read against that harmonic continuum. In fact, it is possible that one could deduce Leibniz's rejection of any attempt to found sovereignty and right on principles extrinsic to "eternal verities" or truths of reason. Koyré remarks that it is difficult to imagine that there was something Leibniz could not understand. Perhaps the nation-state and concomitant theoretical justi-

fications were among the things Leibniz could not, since he would not, follow, because to do so would be much like explanation on the basis of extensive magnitudes alone—mere mechanism.

For the rest, Riley's selections, however dubious their claim to intrinsic merit, are gracefully translated and well ordered. The introduction, furthermore, amounts to a substantial and confident piece of scholarship in its own right. Finally, specialists should also find the critical bibliography useful.

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HARTMUT KAEUBLE. *Berliner Unternehmer während der frühen Industrialisierung: Herkunft, sozialer Status und politischer Einfluss*. With a foreword by OTTO BÜSCH. (Veröffentlichungen der Historischen Kommission zu Berlin, number 40. Publikationen zur Geschichte der Industrialisierung, number 4.) Berlin: Walter de Gruyter, 1972. Pp. x, 302. DM 68.

In his study of Berlin entrepreneurs in early industrialization the author attempts to reorganize some of the traditional concepts applied to the phenomenon. These are treated under two broad subheadings: first, the relationship between entrepreneurship and upward social mobility and, second, the effects of industrialization on the political influence of entrepreneurs. The assumption of authors like Werner Sombart and Joseph Schumpeter that entrepreneurs rose from all classes and were more mobile than now is effectively disproven for the Berlin case by simple statistical analysis. More than half rose from some kind of merchant class, and perhaps one-fourth developed from craftsmen classes in transition to more paraprofessional occupations, for example, in the machine tool industry.

Dr. Kaelble's statistics also make clear that most of the entrepreneurs who originated in Berlin were already mercantile to begin with and that there was very little opportunity for upward social mobility for most of Berlin's citizens. Somewhat less than half of the entrepreneurs migrated to Berlin from other parts of Germany. Many of them were recently emancipated Jewish businessmen and bankers, who made up about half of the middle group among the Berlin entrepreneurs. The author overlooks the fact that upward social mobility does not occur as a result of drastic changes achieved in one generation. According to contemporary sociologists like Bernhard Barber, upward mo-

bility is predominantly to contiguous classes. One could expect most Berliners to aspire to more than a one-step rise only if the educational system provided the opportunity. Obviously it did not. Most of the entrepreneurs studied were near millionaires who rose from a contiguous merchant or banker class. Little was done with the petty entrepreneurs, although some of them seem to have expanded their workshops to middle-sized plants in light industry or in service industries.

As the author is not acquainted with the archival material on economic development for the earlier centuries, he tends too easily to assume that nineteenth-century phenomena herald a new appearance. Thus by the mid-nineteenth century he sees the rise of a new vocabulary and a new consciousness of the factory owner, *Fabrikant*. However, many of the words he selects were already in use in the eighteenth century, both in state papers and in merchant handbooks, whenever there were already factories with machine equipment as, for example, in the early cotton printing industry.

In regard to the political influence of entrepreneurs, the author has a tendency to generalize on the basis of too few facts. He concludes that the entrepreneurs did not withdraw from politics after the Revolution of 1848 because they formed organizations and entered parliament, the Prussian diet. However, the two leagues in question (a free-trade society and a welfare league) failed soon after the Revolution. There was only one entrepreneur in the Prussian diet in the early 1850s, entrepreneurial representation on parliamentary commissions declined markedly, and the Berlin *Kaufmanns-Korporation* fell into the hands of a conservative oligarchy with little real influence. By the 1860s the majority of the Berlin entrepreneurs seem to have been "radical" progressives (favorable to the *Fortschrittspartei*)—a significant change deserving more study and explanation than is offered here.

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ADOLF M. BIRKE. *Bischof Ketteler und der deutsche Liberalismus: Eine Untersuchung über das Verhältnis des liberalen Katholizismus zum bürgerlichen Liberalismus in der Reichsgründungszeit*. (Veröffentlichungen der Kommission für Zeitgeschichte bei der Katholischen Akademie in Bayern. Series B: Forschungen, number 9.) Mainz: Matthias-Grünwald-Verlag, 1971. Pp. xviii, 135.

In the foreword Birke remarks that, despite general realization of Bishop Ketteler's anti-liberal position, studies of him have concentrated largely on his social concerns. Birke undertakes to set Ketteler in place "as representative of the Catholic Church of Germany, in the history of the development of political Catholicism," and to consider how his early antiabsolutist attitude became increasingly anti-liberal.

The book is a well-organized discussion of the problem, much less defensive than Ludwig Lenhart's recent biography of Ketteler. There is a good account of Ketteler's aristocratic antiabsolutism and the maturing of his views of society and politics through study of Thomas Aquinas and contemporary Catholic thought, particularly German and French, and through his participation in public affairs. The study then follows the development of Ketteler, who in the early days of the Frankfurt National Assembly sat with the far Left, to the time when he practically identified liberalism with absolutism.

Despite some new material and careful combing of published sources, the answers are not clear. One asks how "liberal" Ketteler ever was, considering his background and training, and the central role of religion in his thinking and actions. His only speech at Frankfurt dealt with schools; it stresses forcefully German unification—not that the liberals would have disagreed, but that his concept was more romantic than modern, and the Christian-Germanic theme, like his enthusiasm for *Stände* and *Genossenschaften* as forms of social and economic life, suggests updated medievalism. His increasing hostility to liberal individualism and economic self-help, to the "pulverizing of humanity" through unlimited freedom of occupation, trade, and movement, and his view of liberalism as a "mechanically rationalistic concept"—all these from *Die Arbeiterfrage und das Christentum* (1864)—lead to doubts whether he could have allied with liberalism beyond the realm of temporary tactics in which he included political parties, elections, and trade unions.

After 1848 liberalism gradually replaced for him absolutism as the enemy; it combined the mechanical and the bureaucratic, the ruthless centralization of old-style absolutism with egoism in economics and morals; it enforced "liberties" that he regarded as harmful. Somewhat curiously, it merges with his fear of "Borussianism," so that during the Kulturkampf he regarded the attack on the Church as the result of a conspiracy, with Bismarck's support, of Freemasons, Jews, and liberals. The appendix,

incidentally, includes two commentaries by Ketteler on Bismarck's actions at that time, viewed as tactical moves in service of "monarchist-military absolutism."

Large questions remain. Was German liberalism after 1848 so sharply different from before as to justify Ketteler's change of view? Was a lasting Catholic-liberal alliance in Germany really possible? How much needed still is a thorough analysis of German liberalism in the nineteenth century!

REGINALD H. PHELPS
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GÜNTHER BORMANN and SIGRID BORMANN-HEISCHKEIL. *Theorie und Praxis kirchlicher Organisation: Ein Beitrag zum Problem der Rückständigkeit sozialer Gruppen*. (Beiträge zur soziologischen Forschung, number 3.) Opladen: Westdeutscher Verlag. 1971. Pp. 381. DM 24.80.

This is an example of what appears to be a new species of book in the social sciences. Authors and publisher have combined to make it as hard as possible to discover what is being said, why, and on what authority. The fruit of scholarship is hoarded as a treasure and defended against the curious reader with every weapon in the arsenal. Written in atrociously stilted and opaque sociologese, poorly printed from typewriter composition, equipped with a footnote apparatus so bizarre it must be seen to be believed (1.0001.554.0000.0—eleven pages of this, four columns to the page—to 8.1355.049.0204.2), this volume not only makes reading difficult, it positively prohibits it.

But I had agreed to review the book, and so I labored through it. The yield, at least for me, was small, certainly incommensurate with the enormous theoretical and mechanical edifice erected by the authors. What we have is a sociological analysis of the organization of the evangelical church in Germany today, specifically of its increasing alienation from the secularized environment in which it must function. This estrangement provides the authors with their problem. How can the church relate to its surroundings while remaining true to its goals? The authors propose reorganization and provide a model for it, but before they do so they subject the church's present structure to examination. Heavy theoretical artillery is moved up at once (Alvin Gouldner, Talcott Parsons, Amitai Etzioni, Georges Gurvitch, and others) for the obligatory opening chapters on social organization and the theory of organized groups. The relevance of these considerations to

what follows remains as obscure as their language.

What follows in the major part of the book is a detailed description of parochial organization, pastoral duties, and the problems of relating the organization to its functions. Here the authors rest their discussion on interviews with 105 pastors in selected parishes in Württemberg. We discover that rural parishes predominate in the organization, that attitudes prevailing in these shape the ideas of most pastors (hence the *Rückständigkeit* of the subtitle: they lag behind the rest of society), and that this explains the inadequacy of the church to its surroundings (*Umweltinadäquatheit*). Stuck in the rigid frame of its original model, the church cannot relate to the society it wishes to guide. The work of pastors has become so formalized, problems of communication between pastors and people and within the organization so complex (though not too complex to prevent the authors from expressing them in a mathematical formula), and leadership patterns so hierarchical that the bureaucracy has lost touch with real life outside the structure.

All this is set out in massive detail and ponderous prose, enlivened all too rarely by illustrative figures that are fun to read as concrete poetry. The authors' solution to the problem of the church, a restructuring of ecclesiastical organization on the principle of functionality, is displayed in a particularly pleasing diagram on page 326. Cut it out of the book, color some of the little squares with magic markers, and it will be suitable for framing.

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STANLEY B. KIMBALL. *The Austro-Slav Revival: A Study of Nineteenth-Century Literary Foundations*. (Transactions of the American Philosophical Society, New Series, volume 63, part 4.) Philadelphia: the Society. 1973. Pp. 83. \$3.50.

Because of the Enlightenment and especially the Romantic period in literature, several Slavic nations living in the Austrian Empire in the nineteenth century developed a national self-awareness. There is an eleven-page chapter on Serbia, an eighteen-page chapter on Bohemia and Moravia, a ten-page chapter on Croatia, a fifteen-page chapter on Slovakia, and eight pages on the Slovenian awakening. The author explains that a greater variety of organizations accounted for the relatively longer Slovakian

chapter. Since the Poles never experienced an interruption of their language and literature, they are not included.

One should note that the first national language magazine was the Serbian *Ljetopis*, which was printed in Novi Sad. In regard to the tables on pages 71 and 72, I would like to point out that the Serbian organization called *matica* had by far the greatest financial resources. Also, the Czech-Moravian *matice* was the largest one in terms of the number of members. One can also observe peculiar spellings, such as "Sourkromma Spolecnost Ucena," that is, "Private Learned Society," since the Czech language was spoken more in villages than in towns.

In Croatia the *matica* movement first had to decide which of the three dialects—namely Stokavski, Cakavski, or Kajkavski—should become the literary language. It is interesting to note that Emperor Ferdinand forbade the use of the term Illyrian. While the Croats had a relative degree of autonomy, the Slovaks experienced the toughest Hungarian oppression. At that time there were three linguistic orientations in Slovakia. One favored a linguistic union with the Czech language, another favored a western Slovakian dialect, while a third one was that of central Slovakia, which became the basis of the Slovak orthography. Slovenia, which was the smallest Slavic nation living in Austria, had the greatest proportion despite their small number of books published. Also, despite these limited numbers, they still did not display the worst financial resources.

How shall I criticize the book? First, let me stress that Dr. Kimball's visits to Czechoslovakia, Yugoslavia, and Russia were very fruitful. I was impressed by his ability to correctly spell names in different Slavic languages. However, it is probably insufficient to refer to the Ruthenians only on two pages. Since they were a part of the Austro-Hungarian Empire, I feel that more information should have been provided. Second, the nations of Czechoslovakia (the Czechs and the Slovaks) and the nations of Yugoslavia (the Serbs, Croats, and the Slovenians) should also have been discussed. The author could have pointed out more of the similarities rather than the dissimilarities between the Czechoslovak or Yugoslav ethnic groups.

I must, as a final evaluation of Kimball's book, emphasize that I appreciate not only his detailed search for names of the small organizations started by the *matica* in different Slavic nations, but also his description of these groups in terms of financial resources, number of

members, frequency of periodicals, and quantity of books published (as far as one could have established this). This more or less quantitative information gives us not only an insight into Central Southern Europe in the nineteenth century, but it also invites readers to think of similar aspects in other societies of the world in the nineteenth century.

JIRI KOLAJA
West Virginia University

ALEXANDRU DUȚU. *Cărțile de înțelepciune în cultura română* [Books of Wisdom in Romanian Culture]. (Academia de Științe Sociale și Politice a Republicii Socialiste România. Institutul de Studii Sud-Est Europene. "Biblioteca Istorică," 34.) Bucharest: Editura Academiei Republicii Socialiste România. 1972. Pp. 167. Lei 11.

The current concern of Romanian historians with the study of the Enlightenment and the pre-Enlightenment is manifested in Alexandru Duțu's book. Duțu, one of the leading Romanian specialists associated with the Institute of Southeast European Studies of the Romanian Academy, has summarized the latest data on "books of wisdom" published in the Romanian provinces in the seventeenth and eighteenth centuries.

The summary consists of an annotated list of books on deportment, political and civil, a lengthy commentary on works on political theory published in the eighteenth century, and a chronological compilation designed to ascertain the exact dates of composition of the titles listed. Three "complementary studies" on topics peripherally related to the books of wisdom but nevertheless concerned with the Enlightenment round out the volume.

Duțu propounds the theory that the Romanian publications of the pre-Enlightenment and Enlightenment periods, while directly imitative of Western prototypes, contain elements unique to the historic experience of the Romanians. Concern with the role of the individual and of the citizen, for instance, is more pronounced in Romanian than in non-Romanian writings. Similarly, the Romanian historical tradition is evident in the theological rather than philosophical context of secularization.

There can be no quarrel with Duțu's conclusions, which are self-evident. The book, however, is more valuable as an analytical reference work than as a critical interpretation

of Romanian civilization in the seventeenth and eighteenth centuries.

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Istoriia Rumynii [History of Romania]. Volume 1, 1848-1917, edited by V. N. VINOGRADOV *et al.*; volume 2, 1918-1970, edited by N. I. LEBEDEV *et al.* (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 667; 741.

A period of about 120 years of Romanian history is covered in two volumes of over fourteen hundred pages, a massive work by a team of Soviet historians, with space provided for richness of detail and for broad panorama as well as comprehensive surveys and penetrating analyses of events presented in their proper context. This work is a valuable addition not only to Romanian history of the nineteenth and twentieth centuries, but also to the history of the adjoining Central and Southeast European regions. The multilingual bibliography that is included, if added to those already available in English in works dealing with Romania, probably represents a conclusive compilation of sources for the period for the Romanian regions as well as the Banat, Bukovina, and Dobruja, but not for Bessarabia, which is treated somewhat apart.

This work is well organized, and each chronological period is examined from three viewpoints: international affairs and diplomatic history; domestic affairs, government, the history of political parties and movements, and social and economic history; and cultural history. This organization allows for examinations of regional as well as national developments, and this book is given an additional dimension by the inclusion of a number of political developments and movements, especially in the nineteenth century, that remain somewhat tangential in their significance for the history of Romania proper, but are by no means without significance for the history of Southeastern Europe as a whole. The reference here is, of course, mainly to those political movements in the mid- and late nineteenth century, which glowed briefly in the then Habsburg provinces of intertwined ethnicity, so that the movements themselves were a part of the Serbian or Hungarian, as well as Romanian history. This, too, is a valuable contribution.

On the other hand, this work is marred only

too frequently by rather transparent efforts to make past events fit preconceived interpretations. A local strike, a protest march, or an angry article in a local newspaper are made to appear in instances too numerous to list, and in a manner that renders them suspect of pseudodialectical approaches, as harbingers of future revolutionary movements elsewhere or other developments. One wishes that more space and a fuller coverage were accorded, for instance, to Romania's part in the Second Balkan War and to the Treaty of Bucharest of 1913, or to Romania's role in the Little Entente, the relationship between the decline of the Little Entente and the rise of General Ion Antonescu's dictatorship, Romania's adherence to the Tripartite Pact, and her role in World War II.

The Soviet view of Romania's role in World War I, and of the forces that had brought her into the war in August 1916, deserves attention. The blame for ending Romania's neutrality and her declaration of war against the Central Powers is placed on the "Anglo-French insistence" that Romania join the Allies, rather than on the pressures brought to bear upon Romania by the government of Prime Minister Boris Stürmer of Russia. The subsequent, perhaps predictable, chain of events—Romania's quick near-collapse, the doubling of the length of the Russian front, the collapse of tsarism and the start of revolutionary developments in Russia, as well as Romania's separate peace treaty of February 1918 followed by her re-declaration of war in November of that year—is, in a way, made to appear as a product of the alleged "Anglo-French demands." They no doubt existed, but they could not exonerate the Stürmer government of its responsibility for a policy of such far-flung effects.

The flow and ebb of tides of war is unavoidably a major part of any history of Romania. If the beginning of World War I in Romania is discussed in one given manner, the end of World War II is described in details that are not without fascination. The king, a number of generals, and the representatives of the middle-of-the-road political parties developed a feverish, increasingly desperate activity toward the middle of 1944 in an attempt to remove Romania from the Nazi alliance and avert Soviet occupation. After the Red Army's rout of the Nazi armor at the Battle of the Kursk Salient, Romanian envoys in the Near East tried, in moves described vividly, to persuade the Western Allies to dispatch an airborne expeditionary force to Romania. On its

landing the Romanians would arrest Antonescu, declare war on Germany, and thus place themselves on the Allied side. Nothing of course came of this. Their next step was to secure a region centered on Bucharest that would be exempt from the Red Army's occupation—but the text is silent concerning how and from whom this was to be obtained. This failed, too. By August 23, with the Red Army in Romania, the decision to overthrow Antonescu was finally made: "Realizing the hopeless position of the Fascist clique, the king arranged for I. Antonescu's arrest. . . . Under the pretext of a Cabinet meeting, the members of the government were assembled in the palace in the evening of August 23rd, and arrested." It was neither the army, nor the police, nor the royal guards that arrested them. "An armed unit of patriots arrested the Rumanian dictator and his collaborators, and took them from the royal palace to a secret apartment on the outskirts of Bucharest, belonging to the Central Committee of the Rumanian Communist Party, where they were held until they could be turned over to the Soviet command."

Thus came the end of one of the major supporters of nazism in Eastern Europe—taken away by armed patriots through a capital city held by his Nazi masters to a "safe" apartment. Strange endings to a drama can take place anywhere; in Romania, as in Southeastern Europe, they illustrate the intensity and power of forces bearing upon the area from east and west, and north and south—depending on the period of history—and sweeping before them men who tried, often desperately, to stop or to deflect what could be neither halted nor diverted.

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CHRISTOS THEODOULOU. *Greece and the Entente, August 1, 1914–September 25, 1916.* (Hetaireia Makedonikōn Spoudōn, Idruma Meletōn Chersonēsou tou Aimou, 129.) Thessaloniki: Institute for Balkan Studies. 1971. Pp. iv, xxiii, 379. \$5.00.

The war that was to be over by Christmas 1914 had escalated terrifyingly by 1915. To reduce the pressures on the western and Russian fronts, the Entente powers gave much thought to the idea of a third front in the Balkans. Gallipoli was one diversion. Another was raised by bringing Bulgaria in on the

Allied side. This volume deals with the pressures also put on Greece and the difficulties of maintaining neutrality in the face of them. Step by step Greek sovereignty was reduced by the occupation of Salonika, the seizure of Corfu and other islands, the threat of economic sanctions, and the criticism of the king's "un-constitutional" behavior. Constantine was eventually forced to abdicate, and Greece was taken into the war by Eleutherios Venizelos, the mini-imperialist who wanted a greater Greece encircling the Aegean. A Foreign Office official writing in 1915 provides a text that explains why Entente behavior toward Greece was variously threatening, deceitful, cajoling, and insensitive. He wrote that "Bulgaria is the key to the Balkans. . . . Greece will come in if she does." King Constantine did not like it a bit. Neither does Dr. Theodoulou.

His volume is mostly documents, whether in the text or the footnotes. The greater part are from the British Foreign Office archives, released over the last dozen years. French documents are used less because of the "erroneous way" in which the French saw events in Greece. Bolshevik documents are left out because of their "dubiousness." These seem poor explanations. Nor are the archives of the Central Powers used, and this causes difficulties, in spite of the book title, in interpreting the attitudes of the Greek court. The court may have been pro-German or merely neutralist. The documents here do not, and cannot, prove the issue one way or the other.

Theodoulou's use of the documents has led him to provide a commentary, intended as an interpretation but in effect only a rudimentary frame. The material in the footnotes, which are voluminous, often seems to contain more critical material than the text, and it in many places refuses to sustain the argument of the author. By the end of the book one is left wondering if the documents should not have been published in a chronological block, with a detailed introduction. There are many signs of haste, as if the significance of the documents had yet to be fully assimilated before the author began to write. Examples of this are the long footnote on page 26 and also the footnoted material on pages 237 and 251.

The author also overlooks the valuable material already published that would have helped him bring out the views of Lloyd George, Grey, and Joffre, among others, and give life and substance to these people. The occupational risk of diplomatic history is the tendency to disconnect documents from men.

The author proposes to avoid the biases of the Venizelist and royalist camps in Greek historiography, but he often reveals the bias of the indignant patriot. The performance of Entente diplomacy provides ample justification for this. In places, too, the author speaks the language of Venizelos (pp. xxi, 12) and retells many of the irritations all Greeks must have felt with their imperious guests. His views on Napier, Fitzmaurice, and Elliot (pp. 151, 152, 177) are highly partial and suggest an unfamiliarity with these men in other phases of their careers. One hopes, nevertheless, that Theodoulou, having dug so deeply in some archives, will persist in his intention to explore those of the Central Powers. This will allow him to produce a more balanced account than this one and to make a signal contribution to the history of Greece in this highly complex and important period.

The book contains many spelling mistakes and typographical errors, and pages are bound out of order between pages 55 and 72.

ALLAN CUNNINGHAM
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Istoriia rabochikh Leningrada [History of the Leningrad Workers]. Volume 1, 1703–fevral' 1917, edited by S. N. VALK *et al.*; volume 2, 1917–1965, edited by A. R. DZENISKEVICH *et al.* (Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka." 1972. Pp. 554; 459.

The two volumes of this work trace in approximately one thousand pages the experience of the working class of St. Petersburg-Leningrad from 1703 to 1965. It is the product of a research group at the Leningrad branch of the Institute of History of the USSR of the Academy of Sciences, undertaken to remedy the palpable absence of a work so detailed and chronologically extensive, and to set straight the record against the distortions of bourgeois historians of the Russian proletariat. Its function is best understood in the light of a statement in the conclusion that it "reflects the glorious path of the working class of our country, and reveals its role as a leading force of socialist society." One can understand that approach—the working class of Leningrad has indeed played an important part in the history of Russia and the USSR, and can easily constitute individually and collectively the hero(es) of the story—in the early growth of the workers' movement, the Revolution, and during the heroic days of the siege of Lenin-

grad in the Second World War. The names of many of the individual heroes are appropriately sprinkled through its pages.

For a student of Russian history, however, this work is probably of only slight interest or value. The approach is basically narrative and descriptive, with incident following incident for two and a half centuries. There are extensive citations of archives, but insofar as I could judge, these sources seem not to have been exploited to advance any new interpretations. Indeed most of the raw material seems to have been standard sources on Leningrad history. The specialist may well find some nuggets of information—chronologies, names, incidents, etc.—as in the description in the second volume of the counterattack by the Central Committee against the Leningrad opposition at the time of the Fourteenth Party Congress in 1925. But the general tone of the book is to rise above most controversies, taking the standard line on any issue and avoiding polemics.

Indeed what is missing for the generalist is any effort to examine significant theses or interpretations regarding the big themes—the issue of the working class in tsarist development policy (as explored, say, by Zelnik in his *Labor and Society in Tsarist Russia*) or in the struggle over the role to be played by labor unions in Soviet society. In the extensive literature on labor in Russia and the USSR, on economic development, on Leningrad, this seems much less than a landmark work. It attests to the great industry of its authors, but is directed and motivated by no very exciting scholarly quest.

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L. M. IVANOV and M. S. VOLIN, editors. *Istoriia rabocheho klassa Rossii, 1861–1900* gg. [History of the Working Class of Russia, 1861–1900]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 317.

IU. I. KIR'IANOV. *Rabochie Iuga Rossii, 1914–fevral' 1917* g. [The Workers of Southern Russia, 1914–February 1917]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 306.

A. A. MUKHIN. *Rabochie Sibiri v epokhu kapitalizma (1861–1917* gg.) [Siberian Workers in the Period of Capitalism (1861–1917)]. Moscow: Izdatel'stvo "Mysl'." 1972. Pp. 334.

The level of Soviet scholarship in the area of

Russian labor history has been rising significantly over the past several years. Two of the three books under review are illustrations of this welcome trend, while the third reminds us that old constraints continue to be felt, particularly when Bolshevik relations with the labor movement are at issue. Extremely impressive is the collectively written *History of the Working Class of Russia*, of which the "responsible editor" is the late L. M. Ivanov, who has provided so much of the guidance to the recent achievements in labor history within the Academy of Sciences' Institute of History. "Collectively written" is not quite accurate, for, with the exception of chapter 2 ("The Beginning of the Road," which is collectively authored by B. S. Itenberg, the late Iu. N. Shebaldin, and Ivanov), each chapter is attributed to a different author (chapter 1, "The Rise of the Working Class," to Ivanov; chapter 3, "On the Road to Unification with Socialism," and chapter 4, "The Rise of the Mass Social-Democratic Labor Movement," to Iu. Z. Polevoi). Even the collective chapter is divided into sections whose individual authorship can be readily identified by readers familiar with the authors' previous works, and the hand of Ivanov is clearly visible in certain sections of chapters 3 and 4 (notably pp. 152–60 and 258–65). For a collective effort of this kind, the book is written in an unusually smooth and readable style, a pleasant surprise that is probably not unrelated to the authors' relatively flexible and open approach to their subject.

Although footnote references are mainly to primary sources, the book does not purport to be a work of original research, but a synthesis of previous publications by the authors and certain of their colleagues. Indeed, the contribution of Polevoi, which comprises the bulk of the book (pp. 129–308), goes over much of the ground covered in his earlier *Zarozhdenie marksizma v Rossii* (Moscow, 1959), albeit with greater flexibility and thoughtfulness. The authors succeed in covering a chronological range of four decades, all industrial areas of the empire (including those inhabited by national minorities, although these, for defensible reasons, receive considerably less attention than St. Petersburg and the Central Industrial Region), and a broad range of diverse topics such as the transformation of peasants into urban workers, the evolution of spontaneous labor unrest into a politically conscious labor movement, and the parameters of tsarist labor policy. As a synthesis the book is unlikely to be rivaled in the foreseeable future. While to the research scholar it

is no substitute for narrower, more detailed monographs (such as the two other books under review), this is easily the most useful book to assign to graduate students as an introduction to the best Soviet scholarship in labor history.

The overall framework is a familiar one. Reduced to its starkest outline, it is (except for the rigid treatment of "economism") fairly unobjectionable, but not very promising. The emancipation of 1861 launches Russia into the period of capitalism (already anticipated in the 1830s and '40s), laying the groundwork for the transformation of the semipeasant labor force into an economically and socially identifiable but politically still unconscious proletariat ("class in itself" but not yet "for itself") by the late 1880s and early 1890s. In the 1870s a mounting wave of labor unrest accompanies this process, but although the strike now becomes the labor movement's characteristic weapon, its primary features are its "spontaneity," confinement to immediate economic grievances, and narrow isolation to particular factories or, at best, particular regions. Only the most rudimentary expressions of class consciousness begin to appear. Marked by intensive unrest at the Morozov factory (1885) and other textile works of the Central Industrial Region, the labor movement in that part of the country advances in the mid and late 1880s to a new and higher stage, characterized by its stubbornness, the unity and solidarity displayed among workers from different factories, and the more generalized character of workers' demands. But it is only during the industrial upsurge of the 1890s, and especially after 1895, that a combination of objective economic conditions and the influence of Marxist leadership brings about the transformation of much of the working class (the boundaries are never described with precision) into a "class for itself," offensive rather than defensive in its basic demands, national rather than regional in its outlook, and prepared, despite the growing "economist" danger of the late 1890s, to engage simultaneously in political struggle against autocracy and class struggle against capitalism.

The most valuable feature of the book is the authors' willingness to enrich this framework by highlighting the significance of certain "peculiarities" (*osobennosti*) of Russian conditions as determinants of the process they describe. The main "peculiarity" is, of course, the social, economic, and political backwardness of the conditions in which the Russian labor movement evolved. In the words of the authors, "capitalism developed swiftly, but along its

path stood vestiges of a feudal legacy [that is, serfdom and autocracy] which had not been completely liquidated. Autocracy as a form of government, the estate privileges of some, and the people's lack of political rights came into increasing contradiction with the entire course of historical development. Hence the sharpness of social-economic contradictions, the oppressive situation of the masses" (p. 7). Although there are occasional attempts to reduce the concept of *osobennosti* to the truism that there are bound to be some differences in the essentially similar paths followed by the working classes of different countries, these statements are largely *pro forma*. For it is precisely the analysis of uniquely Russian conditions that provides the book with its essential freshness and attractiveness. Having acknowledged the importance of Russia's pre-industrial legacy as an obstacle that the nascent labor movement had to overcome, the authors, however, fall short of taking a final and more daring step: approaching that legacy as a condition which contributed to the successful spread of revolutionary Marxism among the workers of Russia. This suggestion is delicately advanced from time to time (namely, in the discussion [p. 226] of objective conditions that impeded the influence of Russia's "labor aristocracy" and in the conclusion [pp. 304-05]), but the brakes are invariably applied before the argument can be developed fully. It is as if the real challenge were to analyze the unique manner in which the workers of Russia reached a historical moment shared by all the proletariats of Europe, rather than to explain why, in fact, they followed a separate path.

Kir'ianov's detailed study of the workers of the Southern Industrial Region (primarily the Ykaterinoslav, Kherson, and Kharkov provinces and the Don basin, centers of mining, metallurgy, and machine building) covers a later chronological period, the years of World War I to the eve of the February Revolution, but the flexible approach is similar to that of the volume discussed above. (Not surprisingly, L. M. Ivanov was also "responsible editor" of Kir'ianov's monograph.) One of its best features is an explicitly critical attitude toward the earlier Soviet literature on this topic. Thus Kir'ianov takes his predecessors to task, for the most part effectively, for their exaggeration of Bolshevik and denial of Menshevik influence on the working class, their refusal to acknowledge the degree of "chauvinist" or patriotic sentiment among southern workers, their neglect of the spontaneous, economic side of the labor movement, and their overly schematic presentation

of the rise of working-class consciousness under Bolshevik leadership from calendar year to calendar year. He is happy to concede that historians began to retreat from these one-sided positions in the 1960s, but he modestly refrains from noting the important role that his own articles have played in this process. The present work incorporates most of Kir'ianov's previous publications in this area, but it draws upon a wider base of archival and published materials to strengthen his case. He has wisely chosen to organize his materials topically rather than chronologically. He has produced what will become, one hopes, the model for future regional studies of the wartime labor movement in the Russian Empire.

It is unfortunate that Kir'ianov's model is not emulated in Mukhin's study of the workers of Siberia. Mukhin's tone is more reminiscent of an earlier period of Soviet historiography, and his approach to statistical materials, often used as illustrations of a major generalization without regard to the global figures, is less than satisfactory. This is not to suggest that the book is useless. Its strength lies in those sections where the author concentrates on the formation and economic situation of the Siberian working class (primarily railroad workers and miners), its weakness in his discussion of the workers' struggle and the role of political movements, especially Bolshevism. The former sections illuminate the situation of Siberian workers within the context of the region's industrial backwardness relative to other parts of the empire; the motley character and widely dispersed location of Siberian workers are well depicted. The weaker sections roughly correspond to the second half of the book (pp. 171-335), which covers the years from the eve of the 1905 Revolution to October 1917. It is noteworthy that this long chapter, which relies much more heavily than the earlier sections on archival materials, is the most uncritical in its use of sources. Instead of closely analyzing the crucial documents, the author tends to draw from them just those quotations that serve his line of argument. It is impossible to judge from this approach whether conflicting evidence was evaluated and disregarded or simply ignored, but surely the author's conclusion that the workers of Siberia by 1917 constituted a "class for itself" has not been convincingly demonstrated. Indeed, his discussion of the war years, despite the presentation of interesting data here and there, is open to some of the criticisms that Kir'ianov applies to earlier Soviet historiography of the period.

Both of the books favorably reviewed here

would have benefited by dropping certain outdated terminology that is confusing at best, misleading at worst. Why apply the adjective "trade-unionist" to the spontaneous economic struggle of workers who do not even enjoy the luxury of a trade-union movement (*Istoriia rabocheho klassa*)? Kir'ianov similarly detracts from his work when he mechanistically identifies the Mensheviks with the "petty bourgeoisie," especially when one considers how undogmatically he distinguishes the leftist, antiwar segment of the Mensheviks (and Socialist Revolutionaries) from the more moderate, prowar, and defensist tendencies.

REGINALD E. ZELNIK
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S. M. SAMBUK. *Revoliutsionnye narodniki Belorussii (70-e—nachalo 80-kh godov XIX v.)* [Revolutionary Populists in Belorussia (From the 1870s to the Early 1880s)]. (Akademiia Nauk Belorusskoi SSR, Institut Istorii.) Minsk: Izdatel'stvo "Nauka i Tekhnika." 1972. Pp. 244.

This small book is about intelligentsia revolutionists in the Belorussian provinces of the Russian Empire from the 1870s to the mid-1880s. On the one hand Susanna Mikhailovna Sambuk wishes to demonstrate that Belorussian groups were derivative of the Russian Populist movement, and at that time the dominant radical force throughout the Empire. Ideological and organizational leadership came mainly from St. Petersburg radical circles. She is inclined to work from gross and unsubtle generalizations about Russian Populism: for example, that all Populists adhered to the same socioeconomic outlook; almost no alteration or evolution occurred over the years; all Populists denied that the objective conditions for capitalist development existed in Russia; and not a grain of socialism resided in the Populist programs of the early 1880s.

On the other hand she wishes to demonstrate that particular indigenous circumstances shaped Belorussian radicalism and eased the transition to Marxist socialism there. The peasant commune (*obshchina*) hardly existed in these western provinces. Urban workers thus played a greater role in party programs than they did in other provinces. And, more important, Belorussian activists rarely established settlements among the people in the countryside. Instead they preferred to exert influence through intermediaries, usually workers and peasants in the larger cities.

The author is obligated to pass carefully through the treacherous question of Belorussian nationalism. Quotes from the Lenin literary corpus provide judicious aid to navigation. But the wearisome and unhistorical passages where she "corrects" the "errors" of first one then another misguided Belorussian nationalist do not successfully remove those confusions and contradictions caused by her effort to strike the correct balance between Russian revolutionary internationalism and Belorussian national specifics. This is clearly a touchy business. Of particular interest in this regard is her discussion of a largely anonymous organization, *Gomon*, that was criticized at the time—unfairly, she says—for its narrow Belorussian nationalism.

The author devotes attention to the social basis of Belorussian activism. But one wonders about the pertinence of her categories and statistics. Nearly twenty-five per cent of her subjects could not be identified according to "social gradation," and thus appear in her tables as "unknown" or "without a defined occupation." The reader might still choose to applaud her effort. So little social history of this sort is done in the Soviet Union. Especially praiseworthy is her brief search for the factors that compelled individuals to take up the revolutionary cause. Concentrating on A. S. Boreisha, she discovers answers that derive at least in part from the immediate experience of the intelligentsia revolutionists themselves rather than exclusively from the moral outrage occasioned by the suffering of the folk.

Although the author works from a static and simplistic model of revolutionary Populism, she is more discriminating in her view of the historical evolution of Belorussian activism. She places accent on the Populist contribution to Russian Marxist ideology and tactics. Despite the "errors" of the earlier revolutionists, their efforts suggest that the Soviet state possesses what another scholar, G. G. Vodolazov, has called a "native lineage." This is a welcome departure from earlier Soviet historiography that insisted on an unconvincing, sharp disjuncture between Lenin and the traditions he inherited. This, too, is a touchy business.

Much of the author's information is derived from archives and would be more accessible to other researchers if the book possessed an index.

ALAN KIMBALL
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P. F. LAPIN. *Obshchina v russkoi istoriografii poslednei treti XIX—nachala XX v.* [Society in Russian Historiography in the Last Third of

the 19th and the Beginning of the 20th Century]. (Akademiia Nauk Ukrainskoi SSR, Institut Istorii.) Kiev: Izdatel'stvo "Naukova Dumka." 1971. Pp. 297.

L. P. Laptin's book is an involved and tersely written polemic against several prominent nineteenth-century Russian medievalist historians: I. V. Lychintsky, P. G. Vinogradoff, M. M. Kovalevski, and D. M. Petrushevski. The author examines closely the major works of the above Russian historians that are devoted primarily to medieval and eighteenth-century studies. He attempts to show that these medievalists have erred in interpreting the European feudal experience, that they have been too greatly influenced by their own bourgeois prejudices, particularly their fear of the French Revolution and European revolutionary conditions in the late nineteenth and early twentieth centuries.

The author devotes several sections to copious and sometimes confusing quotes from Marx and Lenin in order to present the correct Marxist-Leninist interpretation of his polemic. In my opinion this book should be used for reference only.

HENRY S. ROBINSON
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BRANKO LAZITCH and MILORAD M. DRACHKOVITCH. *Lenin and the Comintern*. Volume 1. (Hoover Institution Publications 106.) Stanford: Hoover Institution Press. 1972. Pp. xiii, 683. \$17.50.

Writing the history of the Communist International presents unusually onerous problems. First of all so many countries, Communist parties, and personalities are involved that few scholars have sufficient knowledge to deal with this complex subject. In addition the archives of the Comintern are securely locked away in the Institute of Marxism-Leninism in Moscow and are inaccessible even to Soviet scholars. Although a great deal of material published by the Comintern and the various Communist parties is available it is often deliberately misleading, or is written in an Aesopian language that is difficult to decipher. Many of the Comintern's activities, moreover, were secret, and information about them was systematically suppressed. Finally, both the revolutionary and the clandestine aspects of the Comintern naturally made it the subject of wild rumors and unsubstantiated stories. In view of these obstacles it is not surprising that few scholars have attempted to chronicle this intriguing world of conspiracies, demonstrations, uprisings, factions, purges, "united fronts," "popular fronts," and

so on. And in fact the best general history of the Comintern, Franz Borkenau's *World Communism*, despite its faults, has not yet been superseded, even though it was published thirty-six years ago.

The present authors have devoted many years to research and writing about the Comintern: Mr. Lazitch is the author of *Lenine et la III^e Internationale, A Biographical Dictionary of the Comintern*, and *Les Partis communistes d'Europe, 1919-1955*, while Mr. Drachkovitch edited *The Revolutionary Internationals* and (with Lazitch) *The Comintern: Historical Highlights*. They have also managed to gain access to many confidential, unpublished documents, such as minutes of meetings of the Presidium and Executive Committee of the Comintern, letters from Comintern officials, manuscript memoirs by important Communists in France and Germany, the personal papers of Boris Souvarine, and the archives of Paul Levi. In addition the authors have built up their own collection of rare Comintern documents, as indicated by the numerous footnotes that end with the comment, "in the authors' possession." (These include such items as original letters by Karl Radek and Clara Zetkin.) And they have benefited from the counsel of former participants in Comintern activities, including Pierre Pascal, M. Goldenberg, Boris Souvarine, and Bertram Wolfe.

It is not surprising, therefore, that this volume is richly documented and abounds in fascinating details. Of particular interest is the new information about some of the underground activities and organizations of the Comintern, including the short-lived Western Bureau, located in Amsterdam, and the more important Western European Secretariat in Berlin, which was scarcely even whispered about in Comintern literature and that has seldom been discussed before by scholars. Equally interesting are the accounts of the official and unofficial representatives that Moscow sent to spy on the leaders of the Communist parties in Western Europe. The long chapter entitled "The Initial Apparatus of the Comintern," which deals with these and other matters, is probably the book's most original contribution.

Less novel are the sections on the early history of the French and Italian parties—subjects that have already been explored in monographs by Annie Kriegel, Robert Wohl, and John McKay Cammett. Still, this is the most comprehensive work yet published on the Comintern as a whole for the years from 1914, when the Third International was just a dream in

Lenin's mind, to the spring of 1921, when he inaugurated an important change of course. The authors promise that a second volume, carrying the story to the death of Lenin in January 1924, will be forthcoming shortly.

THOMAS T. HAMMOND
University of Virginia

K. V. GUSEV and V. P. NAUMOV, editors. *Velikii Oktiabr' v rabotakh sovetskikh i zarubezhnykh istorikov* [The Great October Revolution in the Work of Soviet and Foreign Historians]. (Akademiia Obshchestvennykh Nauk pri TsK KPSS, Kafedra Istorii SSSR.) Moscow: Izdatel'stvo "Mysl'" 1971. Pp. 226.

The October Revolution in Russia has never received the exhaustive and many-sided treatment accorded to the French Revolution of 1789. From this collection of historiographic essays by Soviet scholars it is all too apparent why. Attitudes toward the October Revolution tend to be polarized along political lines. In non-Soviet studies authors, who do not, as a rule, have access to the archives, tend to be hostile toward the event and question its legitimacy or lawfulness (*zakonomernost'*). Soviet scholars by way of contrast, who appear to have all the resources at their fingertips, write one-dimensional political histories in which the Bolsheviks occupy the center of the stage and play all the parts.

There are five essays in this collection, all by leading Soviet scholars. Two of the articles deal with Russian research, the first on the October Revolution in Moscow, and the second on the split in the Socialist Revolutionary party. The other three articles survey foreign scholarship on the October Revolution in France, the United States, and Latin America respectively. Those articles dealing with Soviet works tend to be descriptive, rather than critical, and those treating non-Soviet research tend to measure it by ideological, rather than scholarly criteria.

The essays are interesting to the non-Soviet scholar because they display the trends in research around the world on the October Revolution, and because they put into bold relief some of the most important new directions. The Kachurin essay, for example, on the historiography of the October Revolution in Moscow shows how Soviet scholars since World War II, and especially after 1956, have given more attention to the Provisional Government, the non-Bolshevik parties, and to the local communities—trade unions and district soviets, for example—that made up the city. This is an

updated version of an essay Kachurin published in 1967. According to the editors it was included because there are still many unexplained and debatable issues about the interpretation of the Moscow phase of the Revolution. But Kachurin is not quite candid about the debates, which revolve around those Soviet scholars honest enough to admit that the Moscow Bolsheviks were sharply divided over the need for an insurrection and poorly prepared for the necessary military action.

M. M. Uzakov's essay on American scholarship on the causes of the October Revolution illustrates the general tenor of the three surveys of non-Soviet research. Uzakov correctly observes that most scholars in the United States seek to demonstrate the illegality or illegitimacy of the October Revolution, but he then undermines his case by chastizing Americans for using all the sources, Socialist-Revolutionary as well as Bolshevik. The scholars mentioned in this essay seem to be chosen somewhat at random, and Uzakov makes few qualitative judgments between them. Fainsod, Daniels, and Ulam are lumped together with Tompkins, Ethel Ewing, and Crankshaw.

On balance the two essays on Soviet scholarship are the most valuable parts of the book, chiefly for the convenient inventories of recent Soviet research that they provide.

GEORGE D. JACKSON
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NAUM JASNY. *Soviet Economists of the Twenties: Names to Be Remembered*. (Soviet and East European Studies.) New York: Cambridge University Press. 1972. Pp. ix, 217. \$12.50.

RICHARD B. DAY. *Leon Trotsky and the Politics of Economic Isolation*. (Soviet and East European Studies.) New York: Cambridge University Press. 1973. Pp. 221. \$10.95.

When Naum Jasny died in 1967 he was the *doyen* of Russian emigré economists. His posthumous book on non-Communist economists is organized around the 1931 "trial" of the Mensheviks, though some who did not appear (notably V. A. Bazarov) are also included. This is a very personal book on Jasny's part, especially in its tribute to the memory of V. G. Groman. But its value would also seem to lie in direct proportion to the reader's prior knowledge about the period. It is marred by lapses of fact (such as the assertion that "the peasant land commune" was "introduced into legislation in 1861") that the author would presumably have corrected.

Richard Day has written an extremely interesting study of the political argument on economic policy during the 1920s among the Communists. He postulates alternative strategies for growth as dependent on the basic assumption whether the Soviet economy was to be seen as integrated with, or isolated from, the rest of the world, especially with the tool-producing capitalist West. In particular, Day argues that Trotsky's hard-line approach in 1920 on the mobilization of labor flowed from his belief in the economic isolation of the Soviet republic. During the 1920s Trotsky is supposed to have been converted to the notion that Russia could not be considered in total separation from world capitalism and that she could actually benefit by trade with the West. Day feels as well, quite properly, that "Socialism in One Country" was beneath Trotsky's contempt as a theoretical issue.

The novel framework that Day has established to re-examine the issues of the 1920s works well to explain many points of interest, such as the convoluted positions taken by Sokol'nikov, the commissar of finances. Day is also very good in explicating the real differences between Trotsky and most of the Marxist-Leninist opposition associated with his name. In particular the economic program of Preobrazhenskii is shown to be less a variant on Trotsky's own program than a substitute for it, with little in common between them. Further, while some of the contradictions inherent in "Socialism in One Country" have been evident since the slogan was formed in 1924, Day has made clear that the bloc of Bukharin and Stalin, which made the slogan respectable, was unstable from the very beginning because of the differences fundamentally embedded in the program and not merely because of the incompatibility of its adherents. While Bukharin took the isolation of Soviet Russia to mean the necessity of compromising with the peasant and gradualism, Stalin saw it as the justification for a forced drive of industrialization that far exceeded anything the "Trotskyite" opposition ever anticipated. Indeed, a major contribution of Day's book is precisely to stress the anxiety of Trotsky, in 1926 and 1927, to avoid the anticonsumptionist bent of stepped-up industrialization into which he saw even Bukharin slipping.

While Day's general scheme is conjectural it is also stimulating and enlightening. One part is questionable, namely the idea that Trotsky's pronouncements from exile on the theory of "Permanent Revolution" were a direct falsifica-

tion of his views in 1904–06. There is certainly a hiatus in Trotsky's own interest in the matter, from the Revolution until about 1924. Nonetheless Trotsky at sixty, with whatever fond corrections, could properly claim the stripping of twenty-five as a legitimate ancestor in his opposition to the Stalinist regime.

In any case Day's fine book should help stamp out the illusion that "Permanent Revolution" was an adventurist alternative to the sobriety of "Socialism in One Country." And it should as well generate a discussion to locate those issues in a more fruitful perspective than we have had until now.

DANIEL MULHOLLAND
Tufts University

NEAR EAST

OLEG GRABAR. *The Formation of Islamic Art*. New Haven: Yale University Press. 1973. Pp. xix, 233, 131 plates. \$17.50.

Islamic art is an elusive phenomenon. Since it does not strive for the spatial, the pictorial, the iconographic, or the symbolic density of Western arts, its meaning eludes us. Our problem is further compounded by the baffling diversity of Islamic arts, by the paucity of evidence and the element of chance in our knowledge of artifacts, and finally by the conceptual problems that attach to the term Islamic. What is Islamic art and what is Islamic about it? What are its origins? What does it mean to say? Oleg Grabar, in this book of exceptional subtlety and taste, surveys and extends his own important contributions to the study of early Islamic art history and works out an original and imaginative approach to the elusive and complex problems of understanding Islamic art.

Successive chapters unfold Grabar's way of thinking about the meaning of Islamic art. A historical survey sets Islamic monuments in the context of the Arab conquests and the quality of past civilization in each of the provinces settled by the Arabs. The new art, he hypothesizes, expresses the attitudes, the mentality, the wants, and the needs of a new people. Early Muslim arts represent a "conscious attempt to relate men meaningfully to the conquered world by islamizing forms and ideas of old" (p. 72). Such monuments as the Dome of the Rock and the city of Baghdad are symbols of Muslim supremacy over the religions and empires of the past and of the Muslim appropriation of the past in order to form a specifically Muslim

identity as an outgrowth of the historic cultures.

To express their cultural situation the Muslims created a visual imagery in tension between the old and the new. Grabar reviews their religious and secular creations in thoughtful detail. He shows how the form of the mosque derived from a concept of space for worship that goes back to the time of the prophet Muhammed. The form was now organized by the use of architectural elements borrowed from Byzantine and Christian usage. The use of these elements, however, was governed by a Muslim concern to wrest the architectural motifs from their specifically Byzantine and Christian context of associations, to exclude the use of representations because they were central to the Christian art tradition, and above all to reject the notion that art as such was a symbolic mediation between man and ultimate reality. In Muslim hands the elements of Byzantine and Christian art were organized into a new Gestalt to create specifically Muslim associations and meanings. By contrast secular arts, as illustrated by the Umayyad princely palaces, borrowed Near Eastern motifs of power and luxury without thought of fitting them to Islamic attitudes, but they utilized these motifs as they were utilized in mosque architecture—without great attention to form or to iconographic programs. Functional and social rather than esthetic, formal, or symbolic concerns dominated the structure and decoration of the palaces.

In early Islamic art formal artistic concerns were subordinate to ornamentation. "Total covering, relationships between forms, geometric motifs, infinite potential growth, freedom in the choice of subjects, arbitrariness—such appear to be some of the salient characteristics of early Islamic ornament" (p. 202). What kind of art is this? Grabar sees it as the expression of an attitude toward reality itself. The superficiality, the arbitrariness, and the unreality of the visible world symbolize the transcendence of the divine reality, the Muslim's acceptance that God's ordinance for the order of the world is inscrutable, and his retreat from imitating his maker. Muslim art, he concludes, is essentially an expression of the Muslim attitude toward the very process of creation that underlies the natural universe and the world of man-made artifacts. Given the peculiar nature of Islamic art, the study of stylistic developments, formal problems within the specific genres of art, iconography, and traditions of symbolism—even the study of what place art has in a culture as a whole—can only be realized in conjunction with the study of the functional

significance of art and the mentality that underlies its production and its conceptualization in the mind of the Muslim observer.

At this point the art historian becomes a cultural historian in search of the spiritual universe of Islam. If Grabar has sacrificed, on the one side, some of the art historian's love of specific objects of beauty and, on the other side, some of the cultural historian's inclusiveness of concerns, he has given us a stunning example of the creative possibilities that inhere in the tension between the study of concrete materials in accord with a specific discipline and reflection on the meaning of the specific subject for the civilization as a whole.

IRA LAPIDUS

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OTHMAR PICKL, editor. *Die wirtschaftlichen Auswirkungen der Türkenkriege: Die Vorträge des 1. Internationalen Grazer Symposiums zur Wirtschafts- und Sozialgeschichte Südosteuropas (5. bis 10. Oktober 1970)*. (Grazer Forschungen zur Wirtschafts- und Sozialgeschichte, number 1.) Graz: Selbstverlag der Lehrkanzel für Wirtschafts- und Sozialgeschichte der Universität Graz. 1971. Pp. 366. Sch. 400 (\$16.00.)

Saying that a collection of essays by different authors in a single volume is "uneven" has become so commonplace that one is almost embarrassed to put the word on paper. In the present case the contrasts in quality are particularly striking. On the one hand, Professor Pickl must be commended for publishing some outstanding material. Especially good is his own meticulously documented, well-illustrated contribution, "The Effects of the Turkish Wars on Trade Between Hungary and Italy in the Sixteenth Century." The longest piece in the book, it suggests that financial rather than military considerations brought about a change in commercial patterns. On the other hand, one wonders why the editor even bothered to print half of the other articles included, some of which are only barely relevant to the theme. Was he personally obliged to participants in a symposium for which he was the host?

The prospective reader is perhaps best served by merely listing the titles (sufficiently descriptively per se) of those selections that I found really novel and informative. The honors go to the South Slavs and Magyars, if one excepts Pickl himself, and Hermann Kellenbenz (Nuremberg), who offers a useful, mainly factual summary of secondary literature in "Southeast-

ern Europe Within the Framework of Europe's Overall Economy." Sergej Vilfan's (Ljubljana) "Economic Effects of the Turkish Wars from the Standpoint of Ransom Payments, Taxation and Price Movement" is both brilliant and fascinating. Lajos Rúszás (Pecs) delineates the development of the market village in transdanubia under Turkish rule, while his conational, Ferenc Szakály (Budapest), examines the question of continuity in the economic structure of Hungarian market villages under Turkish control. István Kis (Budapest) turns to the human aspect of economic history in "Society and the Army in Hungary During the Era of the Turkish Wars (the Soldier-Peasants)." Finally, Fedor Močanin (Zagreb) writes on "The Problem of the Landed Property of the Military Population Along the Croatan and Slavonian Frontiers." In summary, the worthwhile features of the book are enough to warrant its acquisition, especially by persons interested in the field but who read only German.

THOMAS M. BARKER

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STANLEY E. KERR. *The Lions of Marash: Personal Experiences with American Near East Relief, 1919-1922*. Albany: State University of New York Press. 1973. Pp. xxv, 318. \$15.00.

The Lions of Marash is an engrossing personal tale of adventure and violence written by a former American relief official and retired chemistry professor at the American University of Beirut. In January 1919 the slim young author left the United States Army Sanitary Corps and began service with the private organization, Near East Relief, at Aleppo, Syria. Kerr transferred to Marash in Cilician Turkey in autumn 1919. The contents deal primarily with events at Aleppo and Marash from January 1919 through July 1920. The book's dramatic focus, to which nearly one-half of its pages are devoted, is a bloody two-month period in the winter of 1920. In that time Turkish nationalists successfully confronted the French occupiers of Marash. Armenians resisted the Turks, having sought independence through French protection and American philanthropic support. During the confrontation Turks killed nearly fifty per cent of the twenty-four thousand Marash Armenians.

The most vital contribution of this work is its extensive narration of the French-Armenian experience with the Turkish uprising at Marash. The numerous specifics provide the

same kind of intensity as Truman Capote's *In Cold Blood*, minus the psychological probing. Kerr's accent is on the wave of destruction as it involves building after building. With the aid of a map of Marash and a portfolio of photographs, the reader witnesses Armenians seeking to defend their most secure houses, churches, schools, orphanages, hospitals, and the quarters of American and European missionaries and relief workers. One Armenian wrote what his Turkish neighbor told him had happened at the house of the Armenian's parents: "We finally broke in. A group of people—fifty or more—were huddled together in one room. Your father asked permission to read from a book and to pray before surrendering . . . He stood up, read and prayed, and then we slaughtered them all—men, women, and children. We didn't waste any bullets on them! We killed them with axes and picks." Kerr quotes an Armenian's diary: "Two days ago the Turks set fire to the orphanage and burned the three hundred fifty orphans in it. Their cries still pierce my heart. We could not go to their aid because of the barbed wire and the enemy machine-gun fire." The partially armed Armenians often inflicted casualties upon Turks. The end of the Armenian resistance came during a frightfully cold three-day period in February 1920 when the French evacuated Marash and one thousand of thirty-four hundred Armenian refugees froze to death in the mountain passes on the road out of Marash.

The title of the book inappropriately implies an evenly balanced impartiality. Lions, the author writes, stand for courageous Turks, Armenians, and French. There are a number of scattered attempts to present Turkish leaders and their views, the chief view being that the nationalist struggle in Marash was the important opening victory in the Turkish drive to eliminate foreigners and their Armenian clients from Anatolia. The author attempts to avoid repeating most traditional pro-Armenian propaganda, as found in such a work as that of his relief colleague at Marash, Mabel E. Elliot's *Beginning Again at Ararat* (1924). One of his problems is that his highly detailed data come largely from his own letters to his family and from memoirs and diaries of French, American, and Armenian people. Partly because he consulted almost no Turkish primary sources, his tale takes on many of the emotions of the non-Turkish materials. Usually avoiding negative stereotypes about Turks (e.g., the terrible Turk label) and simple martyr images

of Armenians, the author also loses much of the pathos found in Elliot's recollection. He might better have edited eyewitness sources on the Marash episode.

Kerr's chief frame of reference for the Marash incident is the military strategy of French and Turks. His thesis is that the French possibly would not have lost Marash if they had possessed a wireless. At the risk of his life the author helped begin negotiations for a French-Turkish armistice, which might have averted a French withdrawal. But just as the negotiations were about to succeed, the French acted on orders to pull back; these orders were not accurate, it later turned out. The inaccurate instructions could have been clarified, withdrawal suspended, and re-enforcements dispatched if there had been a wireless. A counter to this thesis is that the French probably would have lost Marash anyway because of the Clemenceau government's lack of will to defend it and because of the Turks' strength.

The leading difficulty of the book is that the Marash episode is not tied adequately to larger military and peacemaking processes during the First World War period. The complex intermingling of the communal system of the Ottoman Empire with Western intervention is touched upon too lightly. Often the story is a loosely connected set of summaries of different local incidents as recorded by various participants. Some attempt is made at filling contextual gaps by the introduction by historian Richard G. Hovannisian and by the first six chapters, which rather awkwardly prefix the primarily personal description that follows.

The book does not recognize how America's idealistic absorption with the Armenian question and the Wilson government's procrastination in handling it contributed to the Marash situation. Since Kerr directed American relief and married an American missionary he met in Marash, it is interesting that he did not use the latest scholarly works on Near East Relief and on the missionary role in Wilson's Near East policy: Robert L. Daniel's *American Philanthropy in the Near East: 1820-1960* (1970) and this reviewer's *Protestant Diplomacy and the Near East: Missionary Influence on American Policy* (1971).

In sum, the book is valuable for the large number of French, Armenian, and American sources from which it draws, and for a poignant, well-phrased story.

JOSEPH L. GRABILL
Illinois State University

RIAZUL ISLAM. *Indo-Persian Relations: A Study of the Political and Diplomatic Relations between the Mughul Empire and Iran*. (Sources of the History and Geography of Iran, number 32.) [Teheran:] Iranian Culture Foundation. 1970. Pp. xxiv, 287.

This book is Riazul Islam's revised and amplified version of his doctoral thesis for the University of Cambridge, and it deals with the period from 1510, with the Mongol emperor Baber as yet ruling only the small kingdom of Kabul, to the middle of the eighteenth century. It covers a significant chapter in the history of the two nations when India and Iran became great powers under the rule of two outstanding dynasties, the Safawids of Persia and the great Mughals of India.

The intercourse between India and Iran was many-sided, and it covered politics, diplomacy, culture, literature, trade, commerce, and religion. In fact, as the author points out in his preface, "the Mughuls were not involved so deeply with any other foreign power, whether in friendship or otherwise." But the author concentrates only on the political and diplomatic relations of the two dynasties, and he provides an informative, thoroughly documented chapter in a captivating period in the history of two fascinating nations.

Historians have dealt with the Mughal-Safawid relationship, but from William Erskine on, all modern historians of the Mughal Empire have been primarily concerned with a particular period or a particular emperor, such as Sukumar Ray's work on Humayun's sojourn in Persia or the unpublished thesis by M. Jahangir Khan (*The North-West Frontier Policy of the Mughuls*) that deals with Mughal relations with Persia during the reign of Akbar, Jahangir, and Shah Jahan. Mr. Islam's study covers the entire period from Baber to Huḡammad Shah, and in the author's own words, it "treats the subject independently in its own right and not as part of a broader canvas."

The author's use of primary sources, manuscripts, and archival materials is indeed very impressive. The book represents a thorough and painstaking research with some fresh insights and information for scholars in this field. It is not aimed at the casual reader or the layman, and thus one is indeed indebted to the Iranian Cultural Foundation for sponsoring its publication. It is a pity, however, that the author's *Calendar of Documents on Indo-Persian Relations*, often referred to in the text, is still unpublished. The *Calendar* is, from what the author describes, a collection

of numerous letters and documents—scattered in chronicles and manuscripts in various libraries and elsewhere—that have bearing on the political and diplomatic relations between the Mughal emperors of India and the Safawid shahs and Nadir Shah of Iran. The two works are indeed complementary studies, one making a historical study of the subject and the other bringing together the documents on the subject. One hopes that the *Calendar* will soon be published, too, for the sake of interested scholars.

NOSRATOLLAH RASSEKH
Lewis and Clark College

JON KIMCHE. *There Could Have Been Peace*. New York: Dial Press. 1973. Pp. xx, 359. \$8.95.

This is an opinionated and disjointed but enlightening volume by a London journalist who has already published extensively on the Arab-Zionist confrontation. It is not so much a coherent book as a series of "scoops" loosely strung together on the thread of the author's contention that there could have been peace if only Arab and Jew had understood one another and had not allowed themselves to be led down the garden path by the great powers of the day—the British and the French, especially the former, during World War I and its aftermath and by the Americans and the Russians since the Six-Day War of 1967. The first and more interesting section of the volume, "The First Chance" 1917–23, is divided into three chapters. The first retells the familiar story of British Middle Eastern policy with stress on the role of Sir Gilbert Clayton, the director of intelligence in Cairo and the chief political officer with Allenby's forces in Palestine. Kimche advances evidence from the Clayton papers at Durham University to support the argument that Clayton's "grand design" of a new deal for both Arabs and Jews under British tutelage had more substance than the better-known but erratic proposals of Sir Mark Sykes. The second chapter is based largely on revelations made to the author by Israel Sieff, a prominent British Zionist who worked closely with Chaim Weizmann. Kimche takes Weizmann to task for failing to realize, first, that the Zionists could not rely on support from Britain, which had many greater strategic concerns, and, second, that it was imperative to take seriously the burgeoning nationalism of the Arabs in Palestine. The third chapter sketches the hardening opposition of the Palestinians and makes extensive

use of the files and recollections of Aref el-Aref, a close collaborator of Amin el-Huseini, whose political capacity in those early days, Kimche argues, has been unduly overshadowed by his later notoriety as the pro-Nazi mufti of Jerusalem.

After a rather naively muckraking "interlude" on oil diplomacy during and after World War II, part 2, the "Last Chance?" 1967-73, focuses on the domestic and international difficulties of Israel under the premierships of Levi Eshkol and Golda Meir. Kimche contends that both of them, like Weizmann, underestimated Palestinian nationalism and were too ready to cast Israel in the role of a client of the West, in this instance the United States rather than Britain. But the force of his argument is weakened by his reliance on unnamed, highly placed sources, by his hero worship of Moshe Dayan, and by the fact that the outcome of the Yom Kippur War has eroded the author's assumption of Israel's unqualified military superiority.

JOHN B. CHRISTOPHER
University of Rochester

AFRICA

JAMES W. FERNANDEZ *et al.* *Africa & the West: Intellectual Responses to European Culture*. Edited by PHILIP D. CURTIN. [Madison:] University of Wisconsin Press. 1972. Pp. x, 259. \$12.50.

The overseas expansion story so familiar to all up to a generation ago was wholly one-sided because it was told by the conquerors within their own frame of reference. It has changed drastically in our own time with subjectee evidence being increasingly admitted. But one essential element in evolving reappraisals has still been conspicuously lacking in many cases—the weighing of native thought. This must be given full consideration in every discussion bearing upon conquered peoples in all quarters, and, until this receives adequate attention, the revisionist movement must fall far short of its chosen goal. The simple fact is that because of environmental differences, the mental outlooks and thought processes of semi-isolated, underdeveloped groups differ drastically from those of their foreign rulers.

We have here an excellent work on cross-cultural history aimed at establishing perspective and better understanding in studying Euro-African relations. The seven essay authors include anthropologists, historians, and a lit-

erary critic, all associated with the 1969 Conference on African Intellectual Reaction to Western Culture. Selected items will illustrate radically different viewpoints inviting strife between victor and his beaten foe.

Among the Bantu-speaking Fang, who are recent arrivals in the Gabon-Guinea-Cameroons country dealt with by James Fernandez, disputes had traditionally been handled by elders viewing each case as an individual one and rendering decisions accordingly. But alien administrators dispensed "justice" according to rigid lawbooks, a socially irresponsible approach from the local angle. The long resident BaKongo of the subequatorial grasslands discussed by Wyatt Mac Gaffey had completely variant theories of time and space from the Westerners, who concluded that these natives lacked powers of abstract thought and hence were an inferior people.

Africans along the central Atlantic rim were long in close association with outsiders. As Leo Spitzer and Jean Herskovits demonstrate, Europeans viewed the transplanted Western-educated Sierra Leoneans as black Englishmen whose responses must naturally match those of their British counterparts. Yet, as the writings of these authors amply show, the black Englishmen continued under the sway of Yoruba assumptions both in Sierra Leone and in the Lagos settlement. The coastal Senegalese were under powerful French influence for over four centuries, those communally-born actually becoming French citizens. Nonetheless, G. W. Johnson, Jr. illustrates that their intimate ties with indigenous African culture prevented them from becoming fully assimilated Frenchmen. Senghor's poetry is employed by Harold Scheub to illustrate the tragic plight of black Africans seeking in effect to become white Europeans by mere adoption of Western civilization.

Editor Curtin's summarizing chapter holds that thought differences between masters and subject peoples elsewhere have been no less striking than African ones, and he suggests comparative studies on a world basis. This is indeed a provocative book opening a wide field of intriguing potentialities.

LOWELL RAGATZ
Ohio State University

PROSSER GIFFORD and WM. ROGER LOUIS, editors. *France and Britain in Africa: Imperial Rivalry and Colonial Rule*. New Haven: Yale University Press. 1971. Pp. xix, 989. \$30.00.

France and Britain in Africa is a collection of papers presented at a conference sponsored by the Yale University Concilium on International Studies in March 1968. Like an earlier volume, *Britain and Germany in Africa*, produced by the same editors in 1967, it is a massive compilation. There are twenty-four contributors: ten from the United States, five from France, five from Britain, and one each from Belgium, Canada, Nigeria, and South Africa. Most of the current authorities on European imperialism in Africa are represented.

The volume is divided into three parts. The first, "Imperial Rivalry," begins with an overview by Henri Brunschwig of the causes and nature of French Anglophobia in the nineteenth century. This is followed by papers on Tunisia (Jean Ganiage), Egypt (Agatha Ramm), the Congo (Jean Stengers), the Berlin Conference (Wm. Roger Louis), the Fashoda incident (G. N. Sanderson), and the Anglo-French Entente of 1904 (Pierre Guillen). John Hargreaves supplies a short comparative survey of British and French imperialism in West Africa; Colin Newbury an informative and very detailed description of the two powers' tariff policies in the same region; and André Kaspi a discussion of French aims and ambitions in the continent during the First World War. A conclusion to part 1 is provided by Brunschwig, who likens the unfolding of the story of Anglo-French imperialism in Africa to a stage play—a charade, one gathers, rather than a drama—and sums the whole matter up in unequivocally Eurocentric terms: "It thus appears that the partition was completely achieved by Europeans living, thinking, and reacting within their European framework" (p. 405).

Most of the papers that make up "Imperial Rivalry" are exercises in traditional imperial history. We hear of the "official mind," of the "policy makers" among the politicians and bureaucrats in Paris and London, of "men on the spot" like Archinard and Lugard who acted first and sought permission afterward. Several papers draw attention to individual "permanents" employed by one or another home government department who, because of their control of the budget and of the inflow of information from Africa, were able to push through "forward policies" more or less single-handedly, often keeping their colleagues in other departments in the dark about what was going on until it was too late for effective objections to be raised. Above all, the point is repeatedly made that nothing African was ever allowed seriously to jeopardize the es-

sential unity of interest that joined France and Britain together in Europe. Some incidents, such as the British occupation of Egypt in 1882 and the Fashoda confrontation of 1898, provoked a certain amount of sabre rattling, but this was never allowed to go very far. Neither France nor Britain could permit their colonial rivalries to become really inflammatory because the shadow of a united and increasingly powerful Germany lay over them both.

All this is very much the stuff of which the history of European imperialism has been made for the past century or so. There is, it is true, a change of attitude. In contrast to the imperial, and anti-imperial, historians of an earlier day, the writers of these papers adopt a rigorously neutral posture, apportioning neither praise nor blame. Sticking closely to their sources, they are concerned more with chronology and narration than with analysis. As a result, while they supply a wealth of information, much of it new, on the "how" of French and British imperialism in Africa, they do not tackle in any coherent or sustained way the much more difficult question of "why."

Part of the difficulty is that the official sources, while providing a detailed picture of the decision-making process within the relevant metropolitan ministries, do not often indicate what outside influences were at work on the decision makers. When a minister made a public pronouncement on a projected course of action, for example, how much of what he said was rhetoric? And how much was rationalization? To what extent was "the national interest" (so often used as a justification) in fact the interest of a particular pressure group? How many decisions were taken because someone gained the ear of a government decision maker in private and at a crucial time? The absence in the official record of the "monopoly capitalists" of Hobson-Leninist theory does not mean that either they or their influence did not exist.

Part 2 of the volume, entitled "Colonial Rule," contains a set of comparative Anglo-French studies: of military operations in the Western Sudan (A. S. Kanya-Forstner), of the nature and results of economic exploitation (David Fieldhouse), of educational policy (Prosser Gifford and Timothy C. Weiskel), and of the workings of the mandate system (Ralph Austen); articles on the French (William B. Cohen) and British (Robert Heussler) colonial services and styles of government; and four papers that deal with French Africa alone: Boniface Obichere on "the African factor" in

the establishment of French rule in West Africa, Hubert Deschamps (at his best here—magisterial, witty, and anecdotal) on French colonial rule in general, D. Bruce Marshall on the Free French in Africa, and David C. Gordon on Algerian labor in France after the achievement of Algerian independence. The section concludes with a "perspective" by Leonard Thompson. This is a brief but stringent criticism of the approaches his colleagues of part 2 have taken to the topic of colonial rule. Thompson makes three main points: that it is time historians of the imperial era moved the locus of their inquiries from the metropolises to the colonies themselves, that what the French and British thought they were doing in Africa is less important than what they actually did, and that it is the effects of colonial rule—the results of the interaction between rulers and subjects—that should be studied nowadays in preference to "policy" and "method."

Part 3 consists of a bibliographical essay by David E. Gardinier on the literature of French colonial rule in Africa during the period 1914 to 1960. One hundred and sixty-four pages long, it is a comprehensive guide to the relevant source materials and will be of great value to researchers. It is a pity that the editors were not able to arrange for the 1870–1914 period to be similarly covered.

GRAHAM W. IRWIN
Columbia University

MICHAEL CROWDER. *Revolt in Bussa: A Study of British 'Native Administration' in Nigerian Borgu, 1902–1935*. Evanston: Northwestern University Press. 1973. Pp. 273. \$13.50.

BONIFACE I. OBICHERE. *West African States and European Expansion: The Dahomey-Niger Hinterland, 1885–1898*. New Haven: Yale University Press. 1971. Pp. x, 400. \$15.00.

Within the past decade a number of studies have appeared that have illuminated considerably the practice of indirect rule in colonial Nigeria, but none have dealt with its effects in a specific area as fully as Professor Crowder's volume on the brief revolt in Bussa in 1915. The incident, of no great importance to Nigerian history as a whole, is nevertheless of substantial importance as an illustration of how badly a well-conceived theory of governing can work when it falls into the hands of incompetent or careless individuals.

Bussa, the capital of the relatively small

emirate of Borgu on the western boundary of Northern Nigeria, was an unprepossessing little town whose historical importance in pre-colonial times belied its appearance. Its king was an important traditional figure who commanded the allegiance of the rulers of substantially more important centers of the emirate.

With the settlement in 1898 of competing British and French claims to the area, the British portion of Borgu formally became part of the protectorate of Northern Nigeria in 1902, and a resident officer was appointed. Even under the best of circumstances the British presence in the emirate would have been shadowy, since the shortage of officers often meant that one man alone was in charge of the administration over varying periods, with only intermittent assistance. But to add to the problem, the caliber of officers assigned to Borgu province seems to have been frequently comparatively low. In consequence, disturbances occurred that might otherwise have been avoided had administrators of greater perspicacity made more of an effort to understand the people, their customs, and their history.

The author traces in detail the events that led to the rebellion; he makes clear that a combination of mismanagement, not anticipating the results of deposing a ruler, however incompetent he may have seemed in the eyes of the administration, and ignorance of traditional relationships made almost inevitable the violence that followed. Throughout the period the intriguing figure of the ineffective, drunken ruler, Kitoro Gani, appears as a constant theme. Removed by the administration and exiled for a number of years, he was at last brought back to his throne by wiser administrators who realized finally that a traditional chief venerated by his people would be more likely to bring political peace than would any successor selected as a result of British pressure.

Revolt in Bussa is more than a careful and painstaking reconstruction of a vignette of Nigerian history. The author makes of the events a fascinating story, but he also uses them for an examination of the inner workings of indirect rule. By liberal quotation from contemporary administrative reports he brings out the weaknesses of the officers assigned to Bussa during the early years, and he emphasizes the failure of their superiors in the protectorate administration to understand the dangerous course of the actions being reported to them. Crowder's account is all the more vivid to those

who have traveled in the area, but for any student of colonial administrative history it is an invaluable addition to a knowledge of this period.

Professor Obichere's work is the broad canvas of which Crowder has painted a part in detail. Obichere deals with the complex negotiations behind the settlement of British, French, and German territorial claims in the Dahomey-Niger hinterland during the critical period of European expansion between the Congress of Berlin and the Anglo-French Convention of 1898. The problem involved not only international rivalry for trade and territory, but a conflict of personalities in the Foreign Offices of the powers, to which were added the desires of ambitious expansionists of both sides. As Obichere points out, expectations of the wealth of raw materials in the hinterland were greatly exaggerated, often by the leaders of expeditions who sought by this means to reinforce the domestic pressures being brought on the national negotiators to claim as much territory as possible.

The key to development of the area lay in the control of navigation and trade on the lower Niger, and competing commanders sought to supply evidence of positive occupation of territory by means of treaties signed with the indigenous rulers—often with little assurance on either side of the extent of territory covered or the ability of the signers to comply with the terms of the document. But the African rulers were quick to realize the potential value of these treaties as protection against other European invaders, and they learned to play the European diplomatic game in short order.

Much of the material in Obichere's book is already available in other works, but he has gained fresh insights from new documentary sources. His interpretation, stemming as it does from the viewpoint of an African scholar, adds a new dimension to the dry facts of history. It is unfortunate that the volume has the defects which are the inevitable accompaniment of a reworked dissertation—less than vivid writing and an overwhelming burden of academic paraphernalia.

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Albany*

BRIAN WEINSTEIN. *Éboué*. New York: Oxford University Press, 1972. Pp. xiii, 350. \$8.95.

This volume on Félix Éboué fills a long felt and very real need of the English-speaking

student of African history for materials in the English language on the African possessions and leaders of France in Africa. The need is twofold: first, there is a need for general studies on French West and Equatorial Africa and on the French philosophy of colonial administration, and second, there is a need for more specific studies on the leaders who have influenced the formation and direction of the French world order. It is in this latter category that this work on Éboué is a welcomed addition. However, it must be admitted that while the need is clear, it is a question as to whether Professor Weinstein's account is effective in filling in some of that void. Certainly his research was most thorough and his interviews with the members of the Éboué family and with close associates and friends were extensive. It is my fear, however, that Éboué may, in a sense, have escaped the writer. Perhaps a concluding chapter, designed to bring together the attributes, thoughts, and programs of this important French African administrator, might have been helpful.

Mr. Weinstein traces his subject, often with laborious detail, throughout his life, from early childhood in Guyane, through his education in France, through his long colonial administrative career in French West and Equatorial Africa and the French Caribbean, through the significant and tumultuous days of his actions in behalf of de Gaulle, through his appointment as governor-general of French Equatorial Africa, through the Brazzaville meeting, to his untimely death in Cairo in 1944. However, it would have been helpful if Weinstein could have given his reader a deeper insight into Éboué, his feelings, his frustrations, and his thoughts. Also it would seem that the work does not fully explore the pivotal role of the emergence of Éboué as a leader in the events that brought de Gaulle to effective power. A more penetrating study of the Brazzaville conference and its significance would have been especially helpful to the student of contemporary African history.

While this volume has some distinct omissions it is, nevertheless, a most valued addition to the literature of French Africa.

It is to be hoped that this book will encourage others to illuminate further the role of Éboué and other significant figures of French Africa. The book has a good bibliography and index attached.

VAN MITCHELL SMITH
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PHYLLIS M. MARTIN. *The External Trade of the Loango Coast, 1576-1870: The Effects of Changing Commercial Relations on the Vili Kingdom of Loango.* (Oxford Studies in African Affairs.) New York: Oxford University Press. 1972. Pp. ix, 193. \$14.50.

Fundamental questions regarding the course of African history in terms of precolonial trade have been raised in the past decade: the origin and function of domestic slavery and its relationship to the Atlantic slave trade; the pattern of initiative and response in the relations between African states and European traders; and the effect upon indigenous societies of organization of large-scale foreign trade. We are indebted to Professors John Hargreaves and and George Shepperson for another significant contribution in their Oxford Studies in African Affairs. The usefulness of Dr. Martin's book is in focusing these questions on the Loango coast, a relatively small area of western Central Africa north of the Congo River that was marked by political and social cohesion at the time of its first contact with Portuguese traders late in the sixteenth century. Three centuries later, at the point of being overrun by European claims, it was characterized by political fragmentation and social disintegration.

Expanding on her chapter in *Pre-Colonial African Trade* (Richard Gray and David Birmingham, eds., 1970), Martin has carried the story down to the nineteenth century to good effect. Utilizing the theoretical tools outlined by Gray and Birmingham and building on the base established by Jan Vansina in *Kingdoms of the Savanna* (1966), Martin begins with the traditional, hierarchical societies of Loango, Kakongo, and Ngoyo that existed when they asserted independence from the Congo political system south of the Congo River early in the sixteenth century. She then focuses on their commercial relations with one another and with the Europeans, who begin to appear on the Loango coast later that century, illuminating the processes of resulting change by an examination in particular of the Vili kingdom of Loango.

From oral tradition and contemporary observations made by Dutch, French, and English trading agents, officers in the later anti-slave-trade patrols, and missionary writings, a convincing argument is made that the initiative in Afro-European relations was frequently on the African side. Successful adaptation to new external trade demands—such as employing already existing skills in long-distance caravan routes—broadened the base of economic power

as the business of central government increased. The devolution of authority, however, initiated by the once autocratic Maloango, went further than was intended and in the end made his formerly all-powerful position irrelevant. From the beginning and until the mid-eighteenth century, during a long Vili ascendancy over trade on the Loango coast, both the Maloango and his trading manager, the Mafouk, were effective in playing European rivals off against one another and assuring that African control and African pricing for slaves were uniformly maintained.

By the nineteenth century the disintegration of a once-strong African government had dispersed political authority among local brokers, who became petty chiefs. Though this situation was itself a successful adaptation to new commercial needs, it resulted in a system of arbitrary exactions that discouraged economic effort above subsistence level among the common people, adding to the image of a stagnant, backward society that Europeans believed they saw in Africa. The dynamics of these changes are carefully examined in this book, and in an appendix Martin supplies a provisional list of Loango rulers and the sources upon which she bases her analysis. The author has made a substantial addition to the ever-deepening exploration of the African past.

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TADDESSE TAMRAT. *Church and State in Ethiopia, 1270-1527.* (Oxford Studies in African Affairs.) New York: Oxford University Press. 1972. Pp. x, 327. \$17.75.

Beginning with two introductory chapters on Ethiopia before Aksum and the rise and legacy of that state, to which all subsequent dynasties acknowledged their origin, Dr. Tamrat has produced a most perceptive and informative analysis of the rise of the medieval empire of Ethiopia. In striking parallel to feudal Europe in the same centuries, the process of empire building in Ethiopia has rested firmly on the twin pillars of the church and the crown. During the Aksumite period the dynamics of these two institutions produced a tradition of expansion by the book and the sword that continued for centuries until the decline of the empire at the end of the fifteenth century and its near demise between 1531 and 1543 because of the Muslim onslaught under the famous Ahmad Gragn.

The author's narrative and analysis begin in the thirteenth century with the increasing development of church and state under the new dynasty founded by Yikunno-Amlak in 1270. The author, with a combination of keen insight and skeptical skill, has extracted the reality, represented by a dramatic increase in the sources, mostly hagiographical, behind the paeans of praise habitually attributed to the holy and the great. The story unfolds with the majesty and mysticism of royalty and faith. The state evolves by the kings, who steadily assert themselves over their vassals, thereby increasing the wealth at the disposal of the crown. This trend toward centralization was accompanied by a literary awakening in the church that revived monasticism and inspired a series of reforms. Together soldiers and missionaries extended the territorial limits of the Christian kingdom.

It comes as no surprise that the expansion of church and state was most active under the two outstanding military leaders of medieval Ethiopia. Awdā"-Sijoy (1314-44) and Yishaq (1413-30), who pushed the frontiers of Christendom into the Muslim heartland to the south and east and into the Agaw and Falasha country to the north and west. These bursts of military and Christian expansion were followed by periods of consolidation characterized by intense literary and religious activities under such active kings as Zār'a-Ya'iqob (1434-68). A devout Christian, Zār'a-Ya'iqob sought to use Christianity to stabilize the conquests of his predecessors in the pagan and Muslim areas while seeking to bring unity to the diverse peoples and cultures of the empire through fidelity to the one true religion and loyalty to the crown. He failed, not through lack of effort, but by the succession of crises that plunged the empire into civil strife which after fifty years left the Christian empire helpless before the Muslim onslaught.

This is rich history, thoroughly researched and critically presented. Here the reader can discover the continuity and sophistication of the literary, religious, and political development of medieval Ethiopia whose dynasty and traditions remain so much a part of Ethiopia today. Whether they shall prevail before the winds of change in Africa is doubtful, but whatever the force they will not be scattered like dust before the whirlwind.

ROBERT O. COLLINS
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Santa Barbara

NORMAN H. POLLOCK, JR. *Nyasaland and Northern Rhodesia: Corridor to the North*. (Duquesne Studies, African Series, number 3.) Pittsburgh: Duquesne University Press, 1971. Pp. 576. \$15.00.

Britain's entry into the nineteenth-century scramble for African territories was prompted in part by Cecil Rhodes's dream of painting the map of Africa British red along a wide swath from the Cape to Cairo. It was the interfering expansionist tendencies of the Boer republics and Germany that caused Britain to enter and administer Nyasaland and Northern Rhodesia. Both territories are now independent as Malawi and Zambia.

Professor Norman H. Pollock, Jr., who teaches history at Denison University in Ohio, describes the British administration of these two territories from 1875 to 1925. He used mainly Philadelphia library resources plus other depositories to supplement material gathered during several months in Africa. His book is a detailed monographic study of colonial problems. As stated in the preface the author did "not give as much attention to Africans and their internal history as one might wish in an exhaustive treatment." What he does cover includes the historical setting, the establishment of the protectorates, the formation of the British South Africa Company, pacification of contending indigenous peoples, administrative problems, the economic base, and the growing political concerns of European settlers. Chapters are devoted to transportation, which the author sees as the heart of the administrative problem in British central Africa, labor problems, missions and education, and medical care and public health. The author shows the cause-and-effect relationship between administrative theory and what was achieved or not achieved. He tries to avoid harsh condemnation of men of that period from this current vantage point. He also tries to show the connections among climate, topography, political development, the economy, transportation, communication, and ideologies.

The book has a concluding summary chapter, an epilogue that brings the history of the areas up to and beyond independence, an appendix that describes geographical conditions under which the principal tribes live, and a second appendix that presents briefly the underlying causes of late nineteenth-century imperialism in Africa. The footnotes are full, the bibliography is useful as a guide for further study, and the index seems ample.

Professor Pollock points out that thus far in-

dependence has been disillusioning for many inhabitants because preindependence problems remain unsolved. He cautions Africans against retreat into an idealized past that cannot be resurrected or never was and against the tendency to find scapegoats for current ills. What he does call for is a frank look at current problems and cooperation among all citizens to solve them.

FRANKLIN PARKER
West Virginia University

ADRIAN PRESTON, edited with an introduction by. *The South African Diaries of Sir Garnet Wolseley, 1875*. (South African Biographical & Historical Studies, number 11.) Cape Town: A. A. Balkema. 1971. Pp. x, 293. \$11.00.

ADRIAN PRESTON, editor. *The South African Journal of Sir Garnet Wolseley, 1879-1880*. (South African Biographical & Historical Studies, number 12.) Cape Town: A. A. Balkema. 1973. Pp. viii, 359. \$15.80.

It was the practice of Sir Garnet Wolseley, "the ablest and most controversial soldier and colonial administrator of the late-Victorian Empire," to keep a diary record of his numerous campaigns and missions. His five extant journals (Ashanti, Natal, Cyprus, Zululand-Transvaal, and Sudan) constitute, as Professor Preston aptly asserts, a unique account of the conduct of military campaigns and colonial administration throughout the formative period of British imperialism. They reveal the motives and methods of that imperialism, "the almost paranoid atmosphere of intrigue that suffused the making and application of British military and colonial policy," and the character, aspirations, beliefs, and methods of their author.

The Natal (1875) and Zululand-Transvaal (1879-80) journals are undoubtedly crucial to an understanding of Wolseley and his decisive role in South African and imperial affairs. Their value is greatly enhanced by Preston's introductions. Based upon exhaustive research, his analyses of Wolseley's career offer an incisive revisionist interpretation that deserves more extensive review than is possible here. His initial presentation of this interpretation in his introduction to the Sudan journal, *In Relief of Gordon* (1967), was somewhat dissatisfying for its brevity and lack of documentation, but the introduction to the Natal journal provides a lengthy, thoroughly documented, definitive evaluation of Wolseley's pre-1875 career and limited consideration of aspects of its subsequent development. The introduction to the

Zululand-Transvaal journal briefly continues—though, unfortunately, without documentation—the evaluation of Wolseley's career, primarily as it developed within the context of the protracted Anglo-Russian confrontation of the late 1870s.

"No other soldier" of the age, Preston asserts, "had so sustained an impact upon the evolution of British military policy and professionalism"; for all his limitations "Wolseley, for good or ill, influenced every significant decision or development in British imperial strategic policy for over thirty years." But that influence was often negative, divisive, and fallible, and Preston's penetrating assessments of Wolseley as a military reformer and strategist and colonial administrator effectively destroy the uncritical hagiography constructed by earlier biographers.

Wolseley emerges not only as an able and energetic leader but also as an arrogant, authoritarian, superficially successful opportunist and intriguer driven by excessive ambition and chronic insecurity. In his performance in colonial administration—especially in his assault upon the Natal Constitution, his advocacy in 1875 of the annexation of Zululand, his postwar settlement of Zululand, and his intemperate response to Boer nationalism—he displayed imperious and arbitrary behavior, a proclivity for short-term expedients, and an enthusiasm for simple, forcible solutions to complex imperial problems in South Africa. The unfortunate manner in which those problems developed after 1875 was to a considerable extent the product of Wolseley's influence.

Having acknowledged Preston's pre-eminence as an authority on Wolseley, one must note that in regard to the Natal mission the author might have given more attention to the role of Theophilus Shepstone, to his relationship with Wolseley and its ramifications in native administration and Shepstone's subsequent service, and to the substance and significance of Wolseley's warnings that the Boer Republics would never accept confederation unless strangled by British acquisition of the Tugela-Limpopo seaboard. Preston missed opportunities to bring out these and other points with material from Wolseley's correspondence and Colonial Office records and to identify such figures as J. H. Brand, T. F. Burgers, and Robert G. W. Herbert. A half-dozen statements in the introduction are incorrect, and notes 72.51, 130.72, 135.95, 159.1, 187.5, 242.13, and 249.1 contain inaccurate or misleading information.

There is little to criticize in the Zululand-Transvaal journal. Its annotation is thorough,

though there are errors in notes 25.1, 26.8, 36.57, 40.73, and 48.15. There is confusion in the August 7 entry: in line 9 "Mr. T. Shepstone" should be "Mr. J. Shepstone" (Sir Theophilus's brother John). Finally, Preston's assertion that Wolseley was "technically subordinate" to Sir Bartle Frere's civil authority (p. 20) is incorrect. As high commissioner of the Transvaal, Natal, and adjoining eastern regions, Wolseley superseded Frere in those areas and exercised supreme powers, Frere's authority being confined to the Cape. All parties, including Frere, clearly recognized this intentional reduction of his authority.

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ASIA AND THE EAST

SILAS H. L. WU. *Communication and Imperial Control in China: Evolution of the Palace Memorial System, 1693-1735*. (Harvard East Asian Series 51.) Cambridge, Mass.: Harvard University Press. 1970. Pp. viii, 204. \$7.50.

The present study is concerned with the "communication-decision" structure as it evolved in the Chinese government during the period in which the Ch'ing dynasty (1644-1912) reached its high point of power. It is the sort of useful institutional history of premodern China that not only adds to our knowledge of the internal workings of the political system, but also effectively relates it to the social and ideological milieu in which the system functioned.

In nine well-documented chapters Professor Wu's query spans the years 1693-1735. It is significant that the initial date was just one decade after the complete suppression of the last armed resistance against the Manchu conquerors, and now the K'ang-hsi emperor was directing more serious attention to the problems of imperial control beyond the existing channels and methods. He initiated the practice of having selected officials, who were also his close associates, to report to him on local matters through the palace (that is, secret) memorials. By the end of the Yung-cheng emperor's reign in 1735 the practice had been expanded and institutionalized into a regular system that included the new office of the Grand Council (*Chün-chi ch'u*), an organ in the central government that continued to occupy a vital place in policy deliberations until the end of the dynasty.

The palace memorial system itself had evolved primarily out of the deliberative procedures of early Ch'ing, and under K'ang-hsi it remained

largely personal and completely secret, the documents seen only by the memorialist and the emperor himself. The use of the palace memorials was extended in the last decade of the K'ang-hsi reign, as the emperor wished to gain more confidential information during the factional strife at court over the succession to the throne. The Yung-cheng emperor, having been designated heir to the throne at the end of this prolonged struggle, continued and expanded the use of secret memorials. Under him an informal coterie (Wu uses the term "committee") of inner Grand Secretaries was formed who assisted the emperor in dealing with confidential matters; at the same time, high provincial officials were ordered to send secret reports to the throne on a broadened scale. With the "overwhelming amount of raw information" pouring in through the new channels of communication (p. 79) the communication-decision system was not only expanded, but underwent structural changes as well. The final stage of this development was the merging of the functions of the Office of Military Supplies, in the course of the northwestern campaigns in the early 1730s, with the high-level advisory function of the inner Grand Secretaries, thus forming the Grand Council.

Based on a thorough examination of archival and published documents Wu's work provides us with valuable insights into the decision-making process in the Ch'ing political system. One cannot help but think that the value of the book would have been enhanced had the author ventured beyond the dynastic concerns and included within his conceptual scheme some reference to the impact of the Chinese scholar-official syndrome on the communication-decision process: the Manchu emperors, in fact, were not the originators of regular procedures for policy deliberation by high officials; the latter was a fundamental aspect of the technique of government that had enabled a centralized monarchy to operate effectively, for the most part, along with a bureaucracy of educated elite to various extents since the Han times. Although the precise personnel and methods of communication and policy deliberation differed with the passage of time, the tempering of the monarch's absolute power by a countervailing power based on the concept of a meritocracy would seem to have played a large role in the ability of the Chinese system to survive until it was challenged by new problems in the modern era.

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W. E. WILLMOTT, editor. *Economic Organization in Chinese Society*. (Studies in Chinese Society. Sponsored by the Subcommittee on Research on Chinese Society of the Joint Committee on Contemporary China of the Social Science Research Council and the American Council of Learned Societies, 1971-72.) Stanford: Stanford University Press. 1972. Pp. xi, 461. \$16.50.

In many ways *Economic Organization in Chinese Society* is one of the best of the seemingly unending series of symposium volumes that are published in the field of Chinese studies. Although it suffers from variations in the quality of the separate essays that mark all such works, it deals with a relatively clearly demarcated period (the eighteenth to twentieth centuries), has a central theme (the social framework of economic modernization), and a distinctive disciplinary orientation (economic anthropology). The thirteen studies follow a continuum of theoretical sophistication. Thomas Metzger's discussion of the organization of the Ch'ing (1644-1911) salt monopoly gives a good deal of interesting detail, but it is marred by an awkward prose style and is characterized by an eclectic use of social science jargon. Susan Jones contributes a fascinating and well-written description of Ningpo banking during the nineteenth century, but she fails to treat one of the most critical roles of banks in early modern societies: the ability to provide an elastic money supply. As in all her work, E-tu Zen Sun provides a smoothly composed and carefully researched picture of the Ch'ing dynasty silk industry. And Craig Dietrich traces a carefully constructed outline of the technology and organization of cotton production during the eighteenth and nineteenth centuries. Although all four of these essays can be classed as traditional descriptive studies, and would therefore be considered pedestrian by proponents of the "new" economic history, the authors are clearly knowledgeable in elementary theory and avoid the kind of gross errors that frequently appear in earlier work on the pre-twentieth-century Chinese economy.

In his contribution Mark Elvin attempts to use a kind of pseudoeconomics to explain the absence of technological innovation in the early modern Chinese textile industries. According to Elvin, "over all growth was held back primarily by a long-established combination of low farm productivity per capita and very high productivity per acre," thereby precluding the development of surpluses in the agrarian sector that could have generated sufficient demand for cloth so as to stimulate invention. The condi-

tions governing this proposition, which he styles as the "high-level equilibrium trap," are so poorly specified that the hypothesis is difficult, if not impossible to test. At a minimum it assumes, as Elvin acknowledges, that "the country was not just a loosely-knit collection of local economies, but an economic unit effectively integrated by trade." This assumption contravenes common sense. If it were true, there must have been a mobility of resources that even the highly developed Western nations have only recently achieved with the help of modern transportation and communications. Indeed, it can be more logically argued that the absence of integration and barriers against interregional exchange were the chief obstacles to technological innovation. Elvin's piece contrasts markedly with that of Ramon Myers, who uses sophisticated theory to analyze the commercialization of agriculture through a comparative examination of the economic behavior of family farms in prewar China and Taiwan. His contribution is one of the best in the book.

The seven remaining essays are written by sociologists and anthropologists. There is a greater degree of theoretical input in these studies of contemporary local social situations than in the historical pieces, with the exception of Myers's article. But this is primarily because of the adaptability of sociology and anthropology to the interviewing techniques used in Taiwan and Hong Kong. Burton Pasternak contributes an absolutely fascinating analysis of the relationships between changes in agricultural technology in Taiwan and alterations in social structures. Lawrence Crissman is less successful in his attempt to test and invalidate central-place theory in an analysis of marketing on the Ch'ang-hua Plain, Tainan. And Stephen Olsen comes to somewhat inconclusive results in his study of the socialization of economic values. He surveyed students from different occupational groups in three Taipei senior middle schools, and the statistical results allow for varying interpretations.

The last four essays contribute to the debate as to whether noneconomic factors such as values make classical economic theory inapplicable to some past and present societies. Donald DeGlopper, Robert Silvín, and Barbara Ward study business activity in contemporary Taiwan and Hong Kong in order to test the idea that the particularistic values of the Chinese were an obstacle to their making economically rational decisions. All come to more or less the same conclusion as Silvín: "There is little concrete evidence to support the argument

that kinship and other prescriptive ties impair economic activity." Finally, John Pelzel examines economic management of a production brigade in Communist China and finds, once the legal limits are specified, a model of economic action quite in accord with what any economist would expect.

This volume, in conclusion, provides a fairly good sample of the kinds of work presently being done by historians, sociologists, and anthropologists on the early modern and modern Chinese economy. In a cheaper paperback edition the book could provide an excellent text for courses on economic history and development.

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JEAN CHESNEAUX. *Secret Societies in China in the Nineteenth and Twentieth Centuries*. Translated by GILLIAN NETTLE. Ann Arbor: University of Michigan Press. 1971. Pp. xii, 210. \$7.95.

LUCIEN BIANCO *et al.* *Popular Movements and Secret Societies in China, 1840-1950*. Edited by JEAN CHESNEAUX. Stanford: Stanford University Press. 1972. Pp. xii, 328. \$11.95.

Chinese secret societies may be traced back for more than two thousand years, but Professor Chesneau and his colleagues are only concerned here with the role of these societies in the revolutionary social changes in China during the century following the Opium War. Chesneau uses a familiar theme in characterizing the period as one of "intense, modern-style revolutionary struggles . . . aimed at eliminating an *ancien regime* that was degenerate, corrupt, and incapable of standing up to foreigners or of leading the country in the path of progress" (*Secret Societies*, p. 11). He breaks new ground, however, in seeing the history of secret societies in the nineteenth and twentieth centuries "not as a subset of the history of secret societies since antiquity, but as a subset of the general history of China during the nineteenth century" (*Popular Movements*, p. 3). This approach requires its own operational definition of secret societies as "associations whose policies are characterized by a particular kind of religious, political, and social dissent from the established order"—that is, as a discrete subset of the genus "Chinese secret society." The principal merit of this approach is that it adds a new dimension to the historiography of modern China by demanding that the testimony of popular, grass-roots participants in the revolutionary process

be sought out aggressively, admitted to consideration, and fully weighed. In as many words, Chesneau denounces (unnamed) Western Sinologists for "their overriding concern with the Confucian establishment, of which they were in a sense unconscious heirs as a result of the mandarin image imposed on China by the Jesuits" (*Popular Movements*, p. 4). As distinguished from "impeccable 'archive material,'" Chesneau's sources include official documents of the societies, such as proclamations, manifestoes, catechisms, oaths, and rituals; recollections and eyewitness accounts of former society members; surveillance reports by the mandarin and police in China and in Southeast Asian countries where many society members were active; reports of other observers with first-hand information (including some journalists); and commentaries by Chinese political personalities from Hung Hsiu-ch'uan to Mao Tse-tung. Such material is used with discriminating respect for its limitations, and the resulting history is fresh, suggestive, and challenging.

Secret Societies is Chesneau's own monograph, but the text is so heavily interlarded with some sixty substantive extracts from major sources that it also becomes the principal source book of its subject. In *Popular Movements* Chesneau and fifteen other scholarly collaborators of various nationalities—Lucien Bianco, Lilia Borokh, C. A. Curwen, Lev Deliusin, Guillaume Dunstheimer, Winston Hsieh, Georges Jidkoff, Ella S. Laffey, Charlton M. Lewis, John Lust, Mark Mancall, Boris Novikov, Guy Puyraimond, Roman Slawinski, and Frederic Wakeman, Jr.—offer specialized article-length studies of specific aspects of the secret society phenomenon—biographic, cultural, economic, political, regional, and religious, among others. The supplementing apparatus of the volume includes four valuable glossaries: first, a bilingual name list of some 250 secret societies (pp. 291-97); second, a list of some 265 persons connected with secret societies, including Sun Yat-sen, Chiang Kai-shek, and other familiar names (pp. 298-305); third, a secret society terminology of some 231 terms (pp. 306-14); and last, a list of Chinese and Japanese authors whose works in their respective languages are included in the extensive bibliography (pp. 315-16).

Among Chesneau's generalized conclusions (*Popular Movements*, pp. 16-21), the following deserve brief mention: first, secret societies have been "forces of opposition" to established authority; second, their recruitment of coolies and other manual laborers was aided by their own economic activities in salt and other com-

modities, control of public markets, and other profit-making activities, including piracy and brigandage, so that "the history of the secret societies is [also] the history of the formation of the illegal petty bourgeoisie" (as summarized by Feiling Davis, a contributor to the French edition of *Popular Movements*); third, they were closely linked to the "old China" of regionalism and other traditional forces and understood its language; and fourth, the main support for the secret societies came from poor peasants, the urban proto-proletariat, and rural and urban fringe elements (largely displaced peasants that migrated to burgeoning towns and cities). If the societies had no interest in class struggle as such, Chesneaux suggests that they "embodied the two main lines of class struggle that we find in France in 1789 and most other preindustrial societies: the struggle of the bourgeoisie to free itself of feudal economic restraints, and the struggle of the peasants against their feudal masters." As a modernizing force, the secret societies formed a new type of voluntary association for the uprooted peasantry and freed them from lineage, village, and other ascriptive connections, with important consequences for social loyalties. By offering these generalizations in a highly tentative sense, Chesneaux actually appears to be suggesting a new syllabus for the future historian of modern China.

Each of the two volumes is complete in itself, but the full impact of Chesneaux's style and method is best felt when both volumes are read as a unit.

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JESSIE GREGORY LUTZ. *China and the Christian Colleges, 1850-1950*. Ithaca: Cornell University Press. 1971. Pp. xiii, 575. \$16.00.

SIDNEY A. FORSYTHE. *An American Missionary Community in China, 1895-1905*. (Harvard East Asian Monographs 43.) [Cambridge, Mass.:] East Asian Research Center, Harvard University; distrib. by Harvard University Press, Cambridge, Mass. 1971. Pp. viii, 146. \$4.50.

Many college teachers today may still remember the not too distant past when books on missionaries were written by missionaries for missionaries in none too critical fashion. The studies of modern China have since progressed. One piece of evidence is the two books under review; each brings forth a wealth of data and is written with a keen sense of analysis.

In Professor Lutz's broad but detailed account of the Christian colleges from their inception to their disappearance under the Peking regime (perhaps the best part is her description of the inner workings of the missionary groups and their home boards) we learn that, in spite of the great weight of education in the total activities of missionaries in China, teaching was never more than a subordinate means to the end of converting China to Christianity. We also learn that the mission schools had a very low priority in fund allocation and that one reason for not teaching the Chinese classics was their incompatibility with Christian doctrine. None of this was known to the Chinese public even though the topic of missionary education commanded a great deal of attention. Professor Lutz also presents well the specific contributions of the mission colleges to agriculture, medicine, library science, and education for women. Although these are well known the author's documentation is very useful.

One might, however, query the implications of some of the author's remarks. After demonstrating that the missionaries turned to education only after they had failed in winning converts and were in need of a different entering wedge Lutz states flatly that "the colleges originated out of the need of Westerners, not as a result of Chinese need." But does this not come very close to the anti-Christian charge that the schools were tools of Western cultural imperialism?

Again, Lutz insists that the Western presence was a catalyst of Chinese nationalism and that the mission schools "aided the Chinese in defining themselves and in defining the West." The crucial point is what constituted Chinese nationalism. Lutz seems to have a complex view of this matter, for she speaks of a variety of things including national unity, anticapitalism, and the desire to accord power to the state. Yet one might adopt a much simpler view and say that Chinese nationalism came essentially as a reaction against Western encroachment and that unity and state power were merely considered appropriate means for national self-strengthening. If this view is correct then the catalytic role of the missionaries was correspondingly negative. To put it simply, the missionaries might have merely fueled Chinese antiforeignism by their own bad behavior. This possibility is not examined critically by Lutz but emerges clearly from Professor Forsythe's monograph, which is a sociological analysis of the attitudes toward China on the part of 103 Protestant missionaries.

By a careful examination of their own writings Forsythe succeeds in drawing a clear profile of the attitudes of these men and women. The picture that emerges is unflattering by any standard. The missionaries were both bigoted and shallow. They had little knowledge of or interest in China beyond the immediate concern of their work. They had a pervasive stereotype of "the Chinese mind" that was overwhelmingly negative and derogatory. Probably because of their failure as evangelists they could see no possibility of any desirable change emanating from within China; therefore they applauded such events as Japan's victory over China but lamented the relatively peaceful policy pursued by the United States. In a crowning act of cupidity the missionaries used force to collect reparations from rural communities in 1900 and 1901, and according to the eyewitness account of an American newspaperman some preachers even participated in the looting of palaces in Peking.

Writing within the framework of a monograph Forsythe does not deal with the broad significance of his findings. Yet these certainly affect our understanding of the roots of contemporary Sino-Western relations. Unless we assume absolute insensitivity on the Chinese part, it is impossible to deny that the missionaries created ill will among the people they supposedly served. Moreover, it is safe to say that this antagonism far outweighed any good will that came from the contributions of individual missionaries to specific fields, since the public attitude toward a group is always based on its stereotyped behavior rather than on the deeds of a few individuals. If this interpretation holds, then many new avenues of research open up. How much harm did the missionaries do? Did they really change their attitudes after 1905? Above all, what are the factors that govern the success or failure of a missionary movement? In spite of the enormous American investment in missionary efforts, there seems to have been no scientific study of these problems. Professor Forsythe is to be congratulated for his pioneer endeavor.

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E. A. BELOV. *Uchanskoe vosstanie v Kitae (1911 g.)* [The Wu-ch'ang Revolt in China (1911)]. (Akademiiia Nauk SSSR, Institut Vostokovedeniia.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 249.

The task of this book, Mr. Belov carefully explains, is to show the reasons for the success of the Wu-ch'ang uprising on October 10, 1911 (the decisive event of the Hsin-hai Revolution that soon led to the collapse of the Ch'ing dynasty in China), to clarify its moving forces and its aims, to analyze the policy of the Wu-ch'ang revolutionary military government, and to suggest some reasons for the defeat of the bourgeois-revolutionary movement by Yuan Shih-k'ai and his feudal-compradore class. Mr. Belov, after opening with a brief treatment of the historiography of his subject, organizes the book in a chronological manner: from the beginnings of armed struggle against the Manchus in 1906, the preparations for Wu-ch'ang, and the railroad protectionist movements, to the Wu-ch'ang uprising itself, the support it received from other provinces outside of Hupei, and the end of the revolutionary movement in Hupei in 1913. There is much detail, and much interpretation of it, throughout. The book is an impressive historical monograph.

Many of the conclusions that Mr. Belov reaches will not trouble American historians, despite his evident wish to force upon bourgeois historiography the view that Wu-ch'ang was quite accidental. Such a construction of the events of 1911 no longer holds up in the West, if indeed it ever did. Belov's history emphasizes the mass participation in the uprising, particularly by the lower echelons of the army. He makes a convincing case for the programmatic weakness of the bourgeois-revolutionary leaders. He confirms that the T'ung-meng-hui was completely out of the picture in Hupei, and goes on to deplore its moderate and elitist leanings. If the leaders were more in touch with the masses, if their forces had not been partially disbanded after 1911, the story might well have been different. But might-have-beens are absurd, especially to a Marxist historian, and Belov does not dwell on them. The very fact that the revolutionary leaders *were* bourgeois can suffice to explain their naiveté, their ties with landlords and feudal-bureaucratic elements, their fear of the people, and their talk of "one big Chinese family." The revolutionary movement was, in Belov's word, an "ill-assorted" camp that as a whole could overcome its acute class contradictions only insofar as the primary job of dumping the Manchus was concerned. After that it understandably disintegrated.

Mr. Belov's treatment of the declared neutrality of the foreign powers in 1911 is rightly suspicious of their pious invocation of inter-

national legal principles. But he does not consider the possibility that the powers, like the Chinese people, were now ready to see the Manchu dynasty come to a well-deserved end. And Yuan Shih-k'ai, entering in the last pages of this book (as is his custom), is figured by Belov as someone, not who was chosen for his ability to handle foreign powers, but who chose himself on the basis of his ability to handle the capitulationist bourgeois leaders of the revolution. Here there is room for a great deal more study and speculation.

ERIC WIDMER
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ROGER V. DES FORGES. *Hsi-liang and the Chinese National Revolution*. (Yale Historical Publications, Miscellany, 99.) New Haven: Yale University Press. 1973. Pp. xviii, 274. \$12.50.

K. S. LIEW. *Struggle for Democracy: Sung Chiao-jen and the 1911 Chinese Revolution*. Berkeley and Los Angeles: University of California Press. 1971. Pp. ix, 260. \$8.75.

A comparison of these two books validates the old aphorism that the more we know the less sure we are. Each adds materially to our knowledge of the last decade or so of the Ch'ing dynasty and the revolution of 1911-12. But both authors re-examine fundamental questions—who the revolutionaries were, what they contributed to the decisive events of 1911-12, what place the revolution has in the course of Chinese history—and they disagree with each other and challenge existing interpretations.

Roger V. Des Forges has taken the refreshingly novel approach of studying the 1900-12 period through the career of a high Ch'ing official. Hsi-liang (1853-1917) held a variety of key posts and earned the respect of his superiors, who included Chang Chih-tung and the empress dowager, and his enemies, including foreign imperialists and anti-Manchu revolutionaries. Loyal, hard-working, and honest, Hsi-liang grappled courageously with the major issues of the day, struggling to reconcile his love of the past with the needs of the present, his respect for tradition and hatred of foreigners with China's need to change, his loyalty to the state with his devotion to the people. Des Forges demonstrates beyond dispute that right to the very end of the Ch'ing there were officials who possessed vision, integrity, and a large capacity for growth and innovation. Less and less can we think of late Ch'ing officialdom as corrupt and reactionary.

But can we go so far as to consider men like Hsi-liang radical? Des Forges finds that Hsi-liang's life was characterized by "three strategies of resistance, expansion, and radicalism." These developed in three phases from 1900 to 1909, after which "he adopted elements of all three strategies and blended them in a most complex and significant way" during the last eight years of his life. Hsi-liang's many-sided career and personality do not fit very comfortably into this rather contrived pattern, and none of the three "strategies" seem sufficient to define entire phases of his life, but of the three overworked and ill-fitting terms, the least suitable is "radicalism." Des Forges applies this term to so many varied activities and policies that it becomes seriously diluted. It comes to include not only reforms that in the context of the 1900s have generally been considered moderate, but also as passive an act as retirement from public life. There is merit in Des Forges's very interesting attempt to find in Chinese tradition some basis for a modern Chinese radicalism, but Hsi-liang was no radical. Perhaps the clearest evidence is that when the time came to choose in 1911, he acted without the slightest doubt or hesitation: he chose wholeheartedly to defend the dynasty and save the throne.

The different perspectives from which K. S. Liew and Des Forges view the revolution can be indicated by their references to Chang Chien. Chang provides an interesting point of contact between Hsi-liang and Sung Chiao-jen, the revolutionary who is the subject of Liew's book. A leading supporter of the Manchu regime's reform movement, especially its effort to introduce by slow stages a limited form of constitutional and representative government, Chang switched sides two and one-half months after the revolution broke out. Thereafter he acted effectively both to force the abdication of the Manchus and to moderate the revolution. Des Forges finds that Hsi-liang's support for Chang Chien in 1910 is one of the clearest examples of Hsi-liang's growing radicalism; Liew places Chang even in 1912 among those "conservative and non-revolutionary elements" who undermined the short-lived Nanking provisional government. Liew appreciates that the single most tangible achievement of the revolution, the overthrow of the Manchu dynasty, could not have occurred without the participation of men like Chang Chien; but the author also realizes that their participation in the revolution as late as the end of 1911 did not make them radicals. For by the end of 1911

the republic was already being born. In January 1912, when even the reactionary Manchu prince and chief grand councilor I-k'uang had come to favor abdication, Hsi-liang was still proposing drastic last-ditch measures to save the throne.

Des Forges has written a very valuable portrait of an able but essentially traditional late Ch'ing official, a man of no ordinary qualities but well short of greatness, who swam in confusing crosscurrents. One misses in Des Forges's book sharper attention to those currents. Fuller discussion of Hsi-liang's relationship to K'ang Yu-wei and Liang Ch'i-ch'ao would have been especially welcome. Des Forges stresses the man and shortchanges the times. Swept this way and that, Hsi-liang spent a frustrated life (I am less "struck by his achievements" than Des Forges) that ended in 1917 with a melancholy "deathbed memorial" to the child-emperor in his palace exile. Sung Chiao-jen, struck down by an assassin's bullets at the age of thirty, sent from his deathbed a telegram, equally melancholy, to China's new ruler. Readers of both will sense similarities as well as differences between the two men, but I was impressed by the contrast between the one urging his child-sovereign to work hard at "the kingly way" five years after he had been deposed, and the other enjoining Yüan Shih-k'ai to "promote democracy so that Parliament can produce an everlasting constitution." It comes as no surprise that Des Forges sees the revolution as "merely a collapse that brought an end to a decade of ferment and change" that was in turn the product of "a long radical tradition" and "a long rebel heritage." Liew, however, sees 1911 as "a momentous event" that not only was the climax of a century-long process, but was the beginning of the last sixty-plus years of revolution. Liew is the more persuasive, largely because he achieves a better balance between the man and the times. Sung's growth and his effort to plan for postrevolutionary construction, even as he tried, despite his youth and inexperience, to lead a revolution at the same time, receive highly sympathetic treatment. But Liew's focus upon Sung does not preclude some excellent wide-angle shots of the larger movement. Liew occasionally allows himself to become defensive about partisan criticism of Sung, but, on the whole, he provides a very good analysis of the complex and contradictory forces that made up a highly diverse and even fragmented revolutionary movement. Sung's preoccupation with the introduction

into China of Western-style institutions is discussed with particular force. Indeed, one major issue that these books raise in different but equally effective ways concerns the uneasy coexistence between Chinese antiforeignism and China's imitation of foreigners. The foreign powers as both threatening imperialist overlords and admired models loom over the lives of men such as Hsi-liang and Sung Chiao-jen and flit still too silently through the history of the Chinese revolution.

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KENNETH E. SHEWMAKER. *Americans and Chinese Communists, 1927-1945: A Persuading Encounter*. Ithaca: Cornell University Press. 1971. Pp. x, 387. \$10.00.

Shewmaker has provided us with a well-researched and carefully written study of the first round of encounters between Americans and the Chinese Communists. Appearing on the eve of renewed contacts the book was a timely reminder of the twisted paths we had followed in our attitudes toward Mao and his followers. Almost all the men and women of whom Shewmaker writes found the Chinese Communists attractive and were sanguine about the prospects for the Chinese people under Mao's leadership. But by the 1950s reporters who had found Yen-an more to their liking than Chungking were accused of conspiring to deceive the American people, of trying to disguise the Red hordes as "agrarian reformers." In the 1960s scholars praised Tang Tsou for raising the status of these American observers from that of traitors to that of fools who did not understand the insidious nature of international communism. Shewmaker, in addition to recounting the visits and analyzing the reports of his subjects, considers both conspiracy and Tsou theses, offering useful correctives.

Shewmaker focuses on the Westerners, mostly journalists, who visited Yen-an in the years from 1937 to 1945. Edgar Snow is the most familiar of these travelers and he analyzes Snow's work with precision and balance. He demonstrates the absurdity of charges that Snow sought to deceive the American people about the nature of the Chinese Communist movement without ignoring the fact that Snow's writings in the 1940s were colored by his commitment to Mao's cause. He treats Agnes Smedley—an archetype "infantile leftist"

—with warmth and Freda Utley, who provided grist for McCarthyite mills, with understanding. T. A. Bisson and Anna Louise Strong come off less well. Others discussed include James Bertram, Evans Carlson, Haldore Hanson, Philip Jaffe, Gunther Stein, and Theodore White.

Referring to recent studies of Communist-controlled areas in the Yen-an period, Shewmaker concludes that his subjects reported accurately and that their understanding of Chinese communism was closer to reality than that of their later critics. On the central question of why Americans were drawn to Mao and his vision he offers several valuable insights. Their disgust with Chiang and conditions in Kuomintang China derived not only from the corruption, repression, and indifference to human suffering they observed, but also from a reaction to Chiang's style of rule—which they perceived as neo-Confucian feudalism. In Yen-an they found not only honest and humane government, but also modern men with Western values. Mao, Chou En-lai, and Chu Teh were open, informal, direct—and appealed to Westerners repelled by the reserve and ritual they found in Chungking. Forced to choose between the Communists and the Kuomintang, American observers found the Communists closer to their image of themselves: the men most likely to create a modern China in which the hideous suffering of the *lao pai hsing* would end. In 1974 who would gainsay them?

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HUNGDAH CHIU. *The People's Republic of China and the Law of Treaties*. (Harvard Studies in East Asian Law, 5.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xv, 178. \$10.00.

Law in general reflects political reality and objectives. Although much the same is true with other states, the political determination of legal principles is particularly pronounced in the case of China.

The author argues that the People's Republic of China accepts the general principles and practices of international law adhered to by the West and the Soviet Union in particular, at least up to the early 1960s. It can be maintained, however, that due to its numerous and politically inspired qualifications, reservations, and outright disagreements, there can be no definite delineation of a uniform Chinese attitude toward international law. This im-

portant point has not been sufficiently highlighted.

The author belabors the legalistic approach, as if China and the West share a sufficient body of common perceptions dealing with treaty law and the existing differences do not constitute a major rift. Although the book presents a thorough compendium of Chinese legal thought and practices through the 1960s, given the knowledge of Chinese history and its basic policies and problems, simple political inference would be sufficient to discern what the actual views of Peking would be in most cases. For example, and all perfectly understandable, are, first, China's legal argument to invalidate the Sino-Indian boundary; second, Peking's shifting position on the legal status of Soviet treaties with Outer Mongolia; third, the treatment of the most-favored-nation clause; and fourth, China's view of the Taipei-Washington Mutual Defense Treaty as "void" being contrary to the United Nations Charter. In this context Chiu's statement that "[Chinese] practice seems to indicate that revolutionary groups have the capacity to conclude treaties" (p. 13) is simply inane.

China's views on the most-favored-nation clause and the "unequal" treaties are primary indicators of its attitude toward the question of treaty law. The examples of "unequal" multilateral treaties (Nuclear Test Ban and Non-Proliferation) that the Chinese consider "null and void" are enlightening as to China's political considerations. On the other hand, the strange silence of the People's Republic on the Big Five's "veto" in the Security Council seems a clear case of political flexibility in its legal positions (p. 68). Yet the most dramatic fluctuation of Peking's perception is seen in the radically different reactions to the Hungarian uprising (p. 88) and the Czech crisis (p. 66).

Indeed, depending on the nature and intensity of its disputes with the Soviet Union and the détente in general with the West, the legal position of the People's Republic would further change: a radical departure from the Soviet position on treaty law, a gradual convergence with the West's, and increasing support for international organizations.

Given the dramatic changes in China's foreign policy and the relatively moderate domestic atmosphere, it seems reasonable to state that the publication of this book has been too hasty. Its serious failure to consider China's present attitude in the postdétente period severely limits its worth. Besides, the author

tried too consciously to investigate China's "legal" views, while they either do not exist in established form or are in a state of rapid evolution contingent on shifting political perspective. Peking's perception of self-interest, national security, historical views, ideological commitment, differing interpretations of Marxism-Leninism in the context of violent dispute with Moscow, and propaganda motives vis-à-vis the third world merit greater attention.

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BERTOLD SPULER. *The Mongols in History*. Translated by GEOFFREY WHEELER. New York: Praeger Publishers. 1971. Pp. ix, 166. \$8.50.

J. J. SAUNDERS. *The History of the Mongol Conquests*. New York: Barnes and Noble. 1971. Pp. xix, 275. \$10.00.

BERTOLD SPULER. *History of the Mongols: Based on Eastern and Western Accounts of the Thirteenth and Fourteenth Centuries*. Translated from the German by HELGA and STUART DRUMMOND. (The Islamic World. Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1972. Pp. x, 221. \$8.00.

The three books under review were intended by their authors to be read by nonspecialists. It will not be inappropriate, therefore, to consider them from that perspective. Bertold Spuler's *The Mongols in History* is based on a series of lectures presented in France in 1959-60. The translator, Geoffrey Wheeler, has provided a few textual notes and an epilogue. Spuler views the Mongols from the standpoint of the history of Iran, Central Asia, and Russia. The Mongols enter his account with the formation of Chinghiz's steppe empire and the establishment of the khanates. They soon vanish from the scene, however, with the incorporation of their western settlements into the Islamic and linguistically Turkish society of the western steppes. The subsequent history of these largely Turkish heirs of the Ilkhanate and Kipchak and Chagadai khanates is then pursued into the twentieth century. Iran is largely excluded after the beginning of the Safawid dynasty, as is Mongolia from the time of the transfer of the grand khanate from Karakoram to Peking. The East Asian conquests of the Mongols are not discussed. The author has thus chosen to focus his attention on those fields of history that his own research has done much to develop. He writes with

economy and assurance, and he has provided a lucid introduction to his subject. His main interest is in political history: the internal politics of the khanates, Eurasian international politics, and the changing position of the Turkish peoples within the Russian Empire and the Soviet state. Economic factors (chiefly trade), the rivalry of religious establishments, and state religious policies are skillfully integrated into the political history.

J. J. Saunders's *The History of the Mongol Conquests* is differently defined in time and space. The author introduces his work with an essay on Eurasian nomadism and an outline of Turkish history from the sixth to the thirteenth century. His treatment of the Mongol Empire ends with the rise of Tamerlane in Central Asia and the founding of the Ming dynasty in China. In contrast with Spuler, Saunders has undertaken to emulate the continental geographic scope of Rene Grousset's *l'Empire des Steppes* (1948) by discussing the Empire *in toto*. Like Spuler, Saunders appears to be more at home in Islamic West and Central Asia than in Eastern Asia, but he has attempted here to transcend that limitation. He has organized his account around the conquest of the "civilized" poles of the Empire—Persia and China—and the manner in which Mongol rule was terminated in these societies. Russia and Central Asia are offered as counterexamples in which the "semi-civilized" population of the one and the oasis and steppe peoples of the other offered less resistance to the formation of ethnically complex states and therefore remained longer under Mongol or Turkish domination.

Saunders's *History* offers certain advantages to the student looking for an introduction to the Mongol Empire: copious annotation links the text to a very large body of secondary literature, and the bibliography is large enough to be useful for Western-language materials. On the other hand, certain shortcomings of Saunders's book appear in direct proportion to its more ambitious scope. His mode of analysis sometimes seems less a help than a hinderance. His assertion that the Khitan and Jurchen "aped" the "superior culture of China" (p. 43) does not contribute to an understanding of the process of acculturation. Moreover, he does not encourage an intelligent interest in Buddhism when he writes that it "has oscillated between the loftiest theosophy and the most debasing superstition" and that it is "beyond the comprehension of most Westerners" (p. 179). Is Mahayana's *trikaya*, the doctrine

of the three bodies of Buddha, any less intelligible than the Christian Trinity? Also, in the author's consideration of the effects of the Mongol conquests on the history of religions, he introduces a kind of revolving-door principle: Buddhism was "suppressed" in China in 845 and "went underground" (p. 179), "the Buddhists" were "expelled" (p. 181), and "under the barbarian dynasties which succeeded the T'ang, Buddhism reentered China" (p. 179). Buddhism, favored by Kubilai, similarly "seemed destined . . . perhaps even to return to its Indian birthplace, whence it had been expelled" (p. 180). Apart from the fact that Buddhism was not "expelled" from China nor, strictly speaking, from India either, such mechanical reification does not lead to analysis, but inhibits it.

The third title under review, Spuler's *History of the Mongols*, is a collection of thirteenth- and fourteenth-century Mongolian, Persian, Arabic, Syrian, Latin, and French source materials, many of which have not appeared before in English translation. The materials are so divided as to provide a neatly articulated documentary account of the Mongol Empire and an easy reference to specific topics. This volume usefully complements the translations presented in Christopher Dawson's *Mission to Asia* (1955).

Welcome as these contributions are, their limitations call attention to the still unmet need for an integral history of the Mongol Empire. Their methodology is better suited to the study of single civilizations than to the simultaneous examination of several civilizations within one great polity. The fact that the Empire was divided into several khanates and that these soon asserted their independence does not require that they be treated separately. Saunders's distinction between the "civilized" and the other regions of the Empire suggests one step toward a comparative analysis: how does a pastoral-nomadic society interact with a bureaucratic empire? The hypotheses of Owen Lattimore and Wolfram Eberhard, among others, still await application to the history of the Mongol Empire as a whole. Finally, it should be noted that both Saunders and Spuler speculate briefly on the possible relationship between the formation of the Mongol Empire and the subsequent relative imbalance between Western Europe and the rest of the world with respect to wealth and power. Here is a fertile field in which the

formulation and testing of hypotheses have scarcely begun.

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ROBERT H. G. LEE. *The Manchurian Frontier in Ch'ing History*. (Harvard East Asian Series 43.) Cambridge, Mass.: Harvard University Press. 1970. Pp. 229. \$8.00.

"Cradle of conflict" is the description Owen Lattimore applied in the 1930s to Manchuria. Now Robert H. G. Lee has picked up Lattimore's trail and given us an account of the contest for control of the broad, rich, and sparsely settled Manchurian frontier zone, roughly defined by the provinces of Kirin and Heilungkiang, immediately south of the Amur River. The contest Lee describes is not the familiar international one. He is instead preoccupied with the more obscure yet, for the ultimate fate of the region, the more decisive domestic struggle of Chinese settlers against both a rugged frontier environment and a long-hostile Manchu court.

Lee deals in detail with the Ch'ing dynasty's policy of isolating frontier Manchuria from Chinese influence, the gradual abandonment of this policy under pressure of immigration and Russian and Japanese penetration, and the resort to a policy of colonization, a time-honored tool of Chinese frontier defense. Lee offers no clear judgment on whether this reversal of policy should be interpreted as another of those creative responses credited to late Ch'ing statesmen or merely a belated reaction to a situation already out of hand.

The real protagonist of Lee's story is the anonymous, land-hungry Chinese peasant, who contributed to a tenfold increase in the population of Kirin and Heilungkiang between the mid-nineteenth century and the turn of the twentieth. This process of Sinification is the pivot on which the history of modern Manchuria turns and is fittingly the subject of the best chapter in this book.

Lee has culled from Chinese gazetteers, official documents, and travel accounts the details for an often engrossing perspective on frontier life. He excels in evoking the atmosphere of raw frontier towns and wild mining camps peopled by colorful characters—exiled scholars, convicts, bandits, merchants, and poachers. It is unfortunate that this monograph, revised for publication from a Columbia University Ph.D. dissertation, retains some of

the flaws common to that genre. At points the author obscures major themes in a forest of facts, or he resorts to the unimaginative practice of summarizing documents mechanically and in tedious detail. He has also failed to consider his topic in a larger context by comparing it to China's other frontier areas. We can nevertheless be grateful for this noteworthy contribution to our knowledge of Ch'ing China.

MICHAEL H. HUNT
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W. G. BEASLEY. *The Meiji Restoration*. Stanford: Stanford University Press. 1972. Pp. vi, 513. \$17.50.

DAVID WURFEL, editor. *Meiji Japan's Centennial: Aspects of Political Thought and Action*. (Studies on Asia, Second Series, volume 1—1968.) Lawrence: University Press of Kansas. 1971. Pp. xii, 105. \$5.00.

The Meiji Restoration of 1868, like all complex historical events, has defied easy interpretation. Was it the central occurrence in Japan's modern revolution? Did Japan in fact experience a revolution in the French or Russian sense? If not, why not? And if so, what were its causes? What motivated its leaders? And what were its results? Historians in and out of Japan have debated these questions, sometimes with great heat, during the last hundred years. For the Japanese the answers to these questions have had deep political and emotional implications. For to them it matters a great deal whether the Restoration can be looked upon with pride, as the start of a national effort at modernization dedicated to the common good, or with shame, as the beginning of a course of statist development based on repression at home and expansion abroad, leading ultimately to war in the Pacific. When in recent years the Japanese faced the prospect of celebrating the hundredth anniversary of the Meiji Restoration, the nation divided sharply over whether the last hundred years had witnessed a "successful" modernization or whether "the revolution" could only be said to have been achieved in 1946 with the adoption of a new "democratized" constitution.

It is inevitable that the Meiji Restoration should be interpreted in a wide variety of ways. For an event so complex can only be "explained" by the use of formula (a preconception of what is meant by revolution) or by simplification (the selection of one aspect as explanation of the whole). The great prewar

debate among Marxist scholars over whether the Restoration represented an absolutist counterrevolution (the *kōza* argument) or the first stage of a middle-class revolution (the *rōnō* argument) was in large part a controversy over definition of what constituted revolution. The differences that have divided Western writers were mainly over whether the primary motive force was of external or internal origin, whether the modern history of Japan was a story of Westernization or, as E. H. Norman so dramatically described it, the working out of class interests vested in the lower levels of the samurai aristocracy, propertied merchants, and rural landlords.

W. G. Beasley's thoughtful and eminently readable book has the primary virtue of not neglecting the complexities and controversies over interpretation of the Restoration at the same time that it presents a detailed and coherent narrative of how Japan moved from the Tokugawa condition of 1830 (weak, divided, unstable, feudal) to the Meiji condition of the 1870s, by which time a new society was being built upon institutions drastically different from those of the old regime. Yet, as Beasley points out again and again, no revolutionary leadership exercised a conscious and continuous influence on events between these dates. The old order was not pulled down in the name of social equality or specific revolutionary goals, but rather in the name of national defense against pressure from Western powers. There were radical leaders, to be sure, as Marius Jansen and Harry Harootunian have so eloquently demonstrated in their respective studies. But the radical ideologues and men of violent action, while they may have helped to arouse the political consciousness of their compatriots, had burned themselves out long before 1868. The Restoration itself was the work of conservative-minded leaders, "realists, pragmatists, bureaucrat-politicians . . . men convinced that national defense required national unity." These men were of no single class, no single station within the hierarchy of wealth, in fact not even necessarily anti-Tokugawa.

But victory over the Tokugawa made these men responsible for government and above all "for implementation on a national scale the policies that would bring Japan 'wealth and strength.'" In pursuit of this policy the Meiji leaders were pushed increasingly to revolutionary measures. Since feudalism was "an obstacle to national strength, it had to go. . . . Since land tax was an essential resource . . .

landlords got confirmation of their landed rights." Samurai privilege was abolished not in the name of social equality but in order to make possible a national conscript army. And so on.

Did all of this add up to revolution? Beasley answers cautiously. Certainly the Restoration lacked the "avowed social purpose that gives the 'great' revolutions of history a certain common character," and modern Japanese society was one "in which 'feudal' and 'capitalist' elements worked together in a symbiosis dedicated to acquiring national strength." If revolution it was, it could best be called a nationalist revolution, "thereby giving recognition to the nature of emotions that above all brought it about."

This conclusion is not new, to be sure, but Beasley leads us to it with a sure hand and through a great wealth of detail along the way. Based on a comprehensive reading of works in Japanese and English, sensitive to all the nuances of interpretation, Beasley's is a synthetic study of great depth and perception. Yet synthesis is not its only quality. The middle chapters that deal with the politics of the Restoration, and the later ones that trace the post-Restoration policy formation, are based on letters, memoranda, diaries, and observations of and about the main protagonists in the Restoration drama, providing a new intimacy in our understanding of the thoughts and motives of these men. A further dimension is given through the use of British records from Foreign Office archives.

Future historians are not apt to quarrel with the balance and thrust of this work. There is, to be sure, a current trend to make more of the civil war that flared in the aftermath of the Restoration. And the whole subject of popular unrest at the end of the Tokugawa period will need re-examination. One of these days we will know more about what merchants and farmers thought in the 1860s and 70s, and this can then be added to the picture. But until then, Beasley's work can stand as a mature summary of where we stand today in our understanding of the Meiji Restoration.

The second book under review takes us back to the question of the meaning of the Restoration as image and as model for the contemporary Japanese and for the rest of Asia. The volume consists of a series of papers prepared for the Midwest Conference on Asian Affairs in 1968. Taken together, they serve as a reminder that the Restoration must always hold an ambiguous place in history. Marius

Jansen's reflective piece explores the special qualities of the Restoration leaders-turned Meiji oligarchy and why they did not institutionalize their influence beyond their own life spans. Sidney Brown describes the continued use of political assassination in Meiji Japan and how the murderers of Ōkubo became folk heroes. Barbara Teters provides a case study of the difficulties that beset one judge in his effort to uphold the rule of law and judicial independence as part of the newly created constitutional government. Two final papers by Frank Wong and Thadeus Flood deal with the ineffectuality of the Meiji pattern of modernization as a model for either China or Thailand. The Japanese of the Meiji period were themselves too nationalistic and too concerned with their own problems to be effectual exporters. The Meiji Restoration was to that extent confined to its time and its place. Unlike the "great revolutions," it provided no idealized universal message for either the people of Japan or their continental neighbors.

JOHN WHITNEY HALL
Yale University

NOBUYA BAMBA. *Japanese Diplomacy in a Dilemma: New Light on Japan's China Policy, 1924-1929*. Vancouver: University of British Columbia Press. 1972. Pp. 440. \$10.50.

While Professor Bamba does shed some new light on Japan's changing China policy during those overshadowed interwar years from 1924 to 1929 he fails to elucidate much beyond the public statements and behavior of the two foreign ministers of the period. Actually he is groping for a sociopsychological rationalization for his conception of prewar Japan as a case of "national-cultural schizophrenia." This psychosis appears to him to be pre-ordained for nations like Japan that modernize from Western impetus. The very success of modernization, he explains, simultaneously produces antipathetic sentiments based on traditional values and a renewed sense of nationalism. The emotions are then articulated by reactionary leaders bent on asserting to the nation and the world a unique national-cultural identity. They implement illogical policies that, as in Japan's case, culminate in a "dilemma of diplomacy." The nation could neither advance nor maintain its interests, especially in China, short of the ultimate irrationality—war. Blindly these traditionalists led Japan to national suicide.

The bulk of this book is concentrated on the

China policy conflict between the traditionalists personified by a frenetic, reckless premier and concurrently foreign minister, Baron General Tanaka Giichi, and the modernists portrayed by a rational, internationally disposed foreign minister, Baron Shidehara Kijūrō. In the introduction the author repudiates hitherto used partial approaches for understanding national policy and claims an awesome, if inclusive, methodology for this study. In the succeeding three parts are discussed the historical background to the rise of the 1920s traditionalists, biographical and ideological, backgrounds of Tanaka and Shidehara and the antagonistic goals and methods of implementing China policy pursued by both men. A concluding section emphasizes the confusion of a "double diplomacy" that brought suspicion and war to Japan, and it boldly sketches the Dr. Jekyll nature of Shidehara diplomacy and the Mr. Hyde character of Tanaka diplomacy. Indeed Baron Tanaka is depicted as "the vital link between Meiji traditionalists and Shōwa ultranationalists" (p. 367).

Bearing the marks of the Ph.D. dissertation from which much of this work was derived its strengths carry concomitant weaknesses. The promise of an exciting presentation based on such intriguing social science concepts as Professor DeVos's adaptation of selective permeability is not fulfilled. This is so partially from poor handling of ideas and partially from the author's penchant to inundate the reader with details, many of which are irrelevant. Yet there is the excitement in this study of embarking on new theoretical paths for explaining such complex phenomena as interactions between actors' subjective perceptions, as well as the conflict between cultural legacy and the will to progress along nonindigenous lines. This book requires further mulling, considerable pruning, more careful attention to accuracy of facts, and a greater sense of detachment by the author. In the vernacular on our campuses Professor Bamba "hasn't quite gotten it all together yet."

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HAN WOO-KEUN. *The History of Korea*. Translated by LEE KYUNG-SHIK. Edited by GRAFTON K. MINTZ. Honolulu: East-West Center Press. 1970. Pp. xii, 551. \$15.00.

WILLIAM E. HENTHORN. *A History of Korea*. New York: Free Press. 1971. Pp. xiv, 256. \$7.95.

There has long been an acute need for bal-

anced, comprehensive, and scholarly books in English on Korean history that may be useful to historians, political scientists, and other students of cross-cultural studies. The two significant books by Professors Han and Henthorn go a long way in filling this need, certainly from my standpoint as a political scientist. There have of course been since World War II a fair number of Korean history books published in Korea and Japan, among other countries, but they were written in Asiatic languages and have remained untranslated. Relatively brief discussions of Korea in a number of excellent books, including *East Asia* (1970) by Professors John K. Fairbank, Edwin O. Reischauer, and Albert M. Craig, could not be comprehensive enough for many Asianists.

Han's work, originally written in Korean for Korean readers and expertly translated and edited for Western readers, proposes to present "a new history of Korea," as contrasted with unbalanced accounts, and promises to emphasize "the social structures of the past" as distinguished from a "dynastic-centered description of history" (p. v). Henthorn introduces his work modestly by stating that it is intended for "university undergraduates studying the history and culture of east Asia" and that "the structural format" for his work, "such as the use of topical categories and the inclusion of terms in the original and considerable institutional information, has been dictated largely by considerations of the audience for whom this work is intended" (p. vii).

In terms of the periods covered, Han begins with "The Primitive and Tribal Societies," but he subsequently pays considerable attention to the "modern" and "contemporary" periods (pp. 203-509). Han's study covers the period up to 1960 when the "old President [Syngman Rhee] had no choice but to step down" (p. 509) in the wake of the student uprising. Henthorn begins his study with "The Archaeological Record" and brings it to the end of traditional Korea, or roughly the end of the Yi dynasty in 1910. Henthorn's four appendixes are most fascinating, particularly the postscript in which he spells out the "theoretical considerations" underlying his assumptions and interpretations of key features of Korean history (pp. 237-40). Henthorn's summary bibliography (pp. 241-50) also provides a valuable guide to further research on Korean history. Han's bibliography (pp. 516-26) is also valuable, and it contains a fair number of works in Korean as well as Japanese. His index (pp. 527-48) is thorough, and Chi-

nese characters are also given for most of the indexed items. Henthorn's book is also indexed (pp. 251-56).

Han, a Korean who studied in Korea and Japan before he was a research associate in the Harvard-Yenching Institute in 1961, has a comfortable and thorough familiarity with Korean history. His book abounds in keen insights and incisive analyses of some characteristic features of the Korean past. His generalizations, however, sometimes tend to be quite sweeping: "Since Buddhism is not an exclusivist or dogmatic religion like Christianity or Islam, these different [Buddhist] sects managed to coexist [in Korea] fairly amicably most of the time" (pp. 69-70). Han's book also contains some ambiguous and unsubstantiated statements. For instance, while discussing the tribal leagues, he writes that "the ruling class probably had somewhat more elaborate houses. *We know that* they dressed in silk, wore leather shoes, and necklaces and earrings for precious stones. Commoners wore clothes of hemp cloth and straw shoes. The characteristic Korean topknot had appeared, and *our sources also mention* the custom of tattooing" (pp. 36-37; the italics are mine).

Henthorn's work is far more tightly and precisely written and pronouncedly topical in structure. His translations of classical Korean poems, excerpts from ballads, Confucian and Buddhist texts, and the like are skillfully used in many parts of his work (for example, pp. 74, 77, 100, 105, 107, 124-25, 126, 135, 148-49, 180, and 190). Not only do they make the book more readable, but they also highlight his professed emphasis on Korean culture, literature, and ideas. Henthorn furthermore lets Koreans themselves succinctly capture the mood, interest, and "intellectual outlook" of many periods. Henthorn writes, for instance, that "the eminent Koryo Confucian scholar Ch'oe Sungo (927-89) expressed it [the intellectual outlook] in this way in 982: 'Carrying out the teachings of Buddha/Is the basis for cultivation of the self./Carrying out the teachings of Confucius/Is the source for regulating the state./The cultivation of the self is necessary for the future world;/Ruling the state for affairs of the present'" (p. 105).

The assumption upon which Henthorn's book is based "emanates from the view that a concern of the study of history is the analysis of societies, cultures, and human relations through time" (p. 237). On this assumption Henthorn expertly interweaves his discussion of the early Koryo governmental structure (pp.

89-91) with, for instance, brief sections on social structure (pp. 95-96), life in the capital (pp. 101-02), and commercial activities of the elite (pp. 102-03). It might be added, however, that these short topical sections occasionally make the work appear fragmented. Henthorn is ever mindful of "a persistent theme which runs through Korean history, revealed in practically every facet of the national life . . . namely, the native and the foreign, sometimes in harmony, sometimes in conflict" (p. 239). His clear awareness of the larger environment for Korean history is manifested effectively through his chronological overviews that are given at regular intervals. Henthorn's original "interpretation of the meaning of Korea's relations with other states" has been influenced by "the notion of functionalism." He also found it useful to view the Chinese influence on Korea as "a process of acculturation, with the critical variables being language, religious and ethical systems, and, to some extent, technology" (p. 239). I would have liked to read his elaborations on these attempts at original interpretations that are so briefly intimated in his postscript. Henthorn writes that events after the reign of King Kojong (1864-1907) "belong to another era and are properly the subject of another book." It is earnestly hoped that he will write another book on Korean history very soon.

Both books are highly valuable additions to a slowly growing number of scholarly studies on Korea. Both should be on "must" lists for any serious student interested in Korea, and both will definitely benefit all students of Asian affairs.

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M. A. LAIRD, editor. *Bishop Heber in Northern India: Selections from Heber's Journal*. (The European Understanding of India.) New York: Cambridge University Press. 1971. Pp. vii, 324. \$19.50.

"I never saw such prospects before, and had formed no adequate idea of such. My attention was completely strained, and my eyes filled with tears, every thing around was so wild and magnificent that man appeared as nothing, and I felt myself as if climbing the steps of the altar of God's great temple." Such views "most sublime and beautiful" moved Heber as his pony took him toward the snowy peaks of "the glorious icy mountains" of Himalaya. But even more noteworthy was Heber's "strong sense of

the fundamental one-ness of mankind." Except for places and names, one can hardly tell when he was writing about England or India. Thus, of one local community he could write: "They are a modest, gentle, respectful people, honest in their dealings and as remarkable for their love of truth as the Puharees of Rajmahal and Boglipoor"; or, of a Brahman Pandit: "He is evidently a man of considerable talent and extremely desirous to improve whatever opportunities of knowledge fall his way."

Such an observer was Reginald Heber. His impressions of India and its people, along with reflections into his own attractive character that these reveal, are what make *Heber's Journal* so important. An incorrigible romantic who relished traveling in the wilds, Heber could remark, "my life has been that of a Tartar chief, rather than an English clergyman." Yet his tours were undertaken not just for sake of curiosity, but out of a sense of professional duty. He looked at the Empire, not as an imperialist, but as a person always responsible for what was in the interests of the people. Thus he did not hesitate to say what he thought of British failings, especially about arrogance and lack of consideration for the feelings of peoples in India. Heber was an Anglican of moderate, Evangelical sympathies, whose elevation doubtless came in part from his managing the unusual feat, as Dr. Laird aptly puts it, "of pleasing men of most diverse opinions." Heber was a supporter of missionary activity at a time when Protestant work, so long limited to German efforts in Tranquebar and Serempore, had hardly begun its large expansion. But he supported such activity only so long as it was carried on with utmost courtesy and sensitivity to the feelings of local inhabitants. Indeed, what so marked *Heber's Journal* at its first appearance and led so quickly to its republication was its deep interest and sympathetic understanding of human concerns in India. At the same time, by its acute probings, it gave a glimpse of a country that was changing, both in social and economic conditions and in responses to ideas from the West.

Thus the reprinting of this splendid classic is a notable service both to scholars and to general readers. First published in 1828, a year after Heber's sudden death in Tanjore, the *Journal* is a compilation of letters and notes sent to his wife. Selections in this edition, which reduce the text to half its original length, contain choice accounts of the bishop's days in Calcutta (1823-24) and of his travels across Northern India to Bombay. In the excellent

introduction Dr. Laird succinctly surveys early British missionary and Anglican Church history in India and gives a brief biographical sketch of Heber and his significance. Explanatory notes, a short glossary, and a select bibliography are also helpfully provided. Doubtless, Americans will only know of Heber, if at all, by his hymns, some of the most popular in the English language. Yet for scholars Heber on India might be compared to Tocqueville on America.

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DHARMAVIRA. *Lala Har Dayal and Revolutionary Movements of His Times*. Foreword by R. C. MAJUMDAR. New Delhi: Indian Book Company; distrib. by South Asia Books, Columbia, Mo. 1970. Pp. vii, 9-363. \$7.50.

In *Lala Har Dayal and Revolutionary Movements of His Times* Dharmavira explores the life of a Punjabi intellectual whose techniques of building a cadre of political workers through the experience of living in an ashram, of using Indian dress to express respect for his own culture, and of disassociation with the government as a means of protest foreshadowed Mahatma Gandhi's use of similar political instruments two decades later. Har Dayal first worked to mold students in north India into political missionaries around 1908, next moved into Indian revolutionary terrorist circles in Europe, then helped to organize Sikh laborers, Hindu intellectuals, and German money in the United States into the abortive *Ghadr* conspiracy during World War I, and finally, at thirty-five, moved permanently into exile in Sweden and later England and turned to teaching, lecturing, and writing philosophical works.

While the subject is both little studied and fascinatingly complex Dharmavira's work manifests the strengths and weaknesses usually found in the voluminous genre of life and times biographies of Indian nationalists. The major defect is the lack of a definite focus on Har Dayal or of a specific thesis about his relationship to the political development of modern India. The inevitable result is repetition and occasional confusion as the narrative wanders from chronological biography to discussions of political events in India to thematic summaries of Har Dayal's ideas on religion and politics and women. As a consequence the author fails to investigate fully key relationships, decisions, and periods in Har Dayal's life such as his

activities in Germany and Turkey during World War I. Erratic documentation might also possibly be a result of the undetermined orientation of the work. At times Dharmavira buttresses generally accepted points with long quotations from memoirs or interviews with contemporaries and references to files of the Home-Political Department of the Government of India. In other places, however, he denies the assertions of a close associate like Gobind Behari Lal without giving any indication of the evidence on which he bases his conclusions.

The assets of Dharmavira's study include a warm evocation of an unusually colorful individual who exerted a strong personal influence over students and those he reached by the spoken word. Dharmavira is at his best in two areas. His description of the personalities, activities, programs, and linkages of Indian revolutionary terrorists in India, Europe, and North America is crowded with illuminating detail not readily available in one source. His survey of Har Dayal's years in exile cover a period of growing isolation from Indian politics. Though Dharmavira does not always explain satisfactorily the contradictions between the thought and actions of his subject his book reveals a man most worthy of further study.

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JUDITH M. BROWN. *Gandhi's Rise to Power: Indian Politics 1915-1922*. (Cambridge South Asian Studies, number 11.) New York: Cambridge University Press. 1972. Pp. xvi, 384. \$19.50.

FRANCIS G. HUTCHINS. *India's Revolution: Gandhi and the Quit India Movement*. Cambridge, Mass.: Harvard University Press. 1973. Pp. ix, 326. \$14.00.

There is much to praise and criticize in these two books by young scholars. Praiseworthy is their use of unpublished private papers and government records in the New Delhi and London archives. Brown also makes good use of recent Ph.D. theses done at Australian National University, Cambridge, and Oxford, while Hutchins was able to interview participants in the 1942 Quit India Movement. Both books, however, show only a partial understanding of Gandhi's motives, goals, and methods, and they seem to me flawed by interpretive schema that distort the realities the authors wish to describe and explain.

Brown, writing as a political historian interested in the interplay between provincial

and national leaders, displays an exemplary attention to detail and an admirable skill in weaving together evidence from British and Indian sources. After an unfortunately brief chapter on Gandhi's twenty-year career in South Africa, closer attention to which would have helped her grasp better what Gandhi was trying to do in 1919-22, she shows how odd he seemed to the Western-educated political elite during his first years back in India (1915-19). Her next chapters demonstrate Gandhi's use of satyagraha (in the form of noncooperation with British-dominated institutions and nonviolent disobedience of selected laws and official orders) to rectify local injustices in rural areas of Bihar and Gujarat and to protest the repressive Rowlatt Bills in 1919. Lieutenants, variously termed "mobilizers," "subcontractors," and "henchmen," were recruited and trained during these struggles in provinces previously little involved in nationalist politics, and these men became the nucleus for one of Gandhi's three "bases of power" when he came forward in 1920 with a program for nationwide satyagraha.

Brown portrays convincingly the crucial role of the second large group supporting Gandhi (because he supported them)—Muslims anxious to protect the holy places of Islam and the status of the defeated Ottoman caliph. The author's detailed narrative ends with Gandhi's victories at the Congress sessions of September and December 1920, the defeated, old-style Congress leaders becoming his temporary allies and the third "base" of his "power."

So far, so good. But in her two concluding chapters Brown seems to me a prisoner of a mechanistic view of politics as the quest for power rather than for justice, national self-respect, or the general welfare. Succumbing to a naive empiricism, she believes her portrayal of the 1920-22 noncooperation movement, based on abundant reports and memoranda of worried British officials and the writings of Indian leaders critical of Gandhi's methods, to be more "realistic" than the accounts of unnamed writers of "nationalist mythology and hagiography." Small wonder that her final paragraph describes Gandhi as "a political failure." What moved Gandhi to come forward when he did, why so many of his countrymen rallied to his support, and what the sacrifices they made in 1920-22 meant to them then and thereafter—such questions are not answerable within Brown's limited framework.

Hutchins's *India's Revolution* rests on a narrower base of evidence but ranges much

more widely in space and time than Brown's *Gandhi's Rise to Power*. Hutchins is at his best in his earlier chapters, which are philosophical essays reminiscent of Aristotle's in that they are gracefully written and studded with both brilliant insights and improbable generalizations. Sensitive to states of mind, Hutchins traces a pattern of change from manipulating one's foreign rulers, to selectively assimilating their values, to rebelling against those values. The fourth stage he posits, revolution, is too broadly defined to be usable as an analytic tool. There is a difference between ousting a foreign power and overthrowing an indigenous ruling group.

In the latter half of his book Hutchins tells us, somewhat disconnectedly, about the 1942-43 outbreaks of lawlessness triggered by the arrests of Gandhi and the Congress leadership. The author argues that what both Gandhi and the Viceroy called "rebellion" (pp. 195, 282) was a "spontaneous revolution" (chapter 9), a "final showdown with the British" in which the Congress "gambled—and won" (p. 273). Just what changes these events produced in the minds and hearts of the people of India and their rulers is not clear from Hutchins's scanty evidence on this question, but his conclusion that they "brought British rule to an end . . . the revolution had triumphed" (p. 283), seems improbable in the light of Jawaharlal Nehru's verdict that "it was a foolish and inopportune challenge."

Both books thus present much interesting material, but they are marred by simplistic or faulty generalizations. Brown seems to underestimate the psychological and political effects of Gandhi's leadership; Hutchins overestimates them. Neither book has an adequate index with respect to the themes and concepts deployed (for example, imperialism, nationalism, power, rebellion, revolution). What is most valuable in both books is the focus on a limited period in Gandhi's career. Brown's has the added merit of discovering detailed evidence of his changing relations with lesser-known provincial and local leaders.

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NALINI RANJAN CHAKRAVARTI. *The Indian Minority in Burma: The Rise and Decline of an Immigrant Community*. With a foreword by HUGH TINKER. New York: Oxford University Press, for the Institute of Race Relations, London. 1971. Pp. xxiv, 214. \$10.50.

The story of the Indian minority in Burma as

it fared from 1852 on is fascinating and melancholy. A small alien minority makes truly outstanding contributions to the economic development of a remote area of a far-flung empire and in time that minority community is inevitably rejected and expelled. The Indian immigrant, as transient or permanent resident, came to Burma at British behest to take up the modern tasks that were not being done by the local population, tasks that lacked prestige in the traditional society. He came as a miserable coolie, soldier, trader, administrator. He stayed on and prospered, and so did the country. Almost never was he in competition with the indigenous peoples of Burma: the Burmans, Shans, Karens, Kachins, and Chin, to name the most important of the ethnic communities.

The immigrant the world over follows this pattern. Forced from his home by economic hardship or some sense of oppression and attracted by economic opportunity, he starts at the bottom of the social ladder. He works his way up in the community through diligence and hard work, and he is ultimately absorbed into his adopted society. The failure to accept this assimilation has been the flaw in the colonial migrations of the nineteenth and twentieth centuries, and the flaw in the life-style of the Indian minority in Burma. The Indians came to the country at foreign request in numbers dictated by foreign authorities, political and economic. They chose, even those who stayed on to become important in the colonial community, to be Indians first and Burmese last. That is to say, the Indian minority with which Dr. Chakravarti's study is concerned never became Burmese. Others from India have done so over the centuries—the Burmese Muslims, Arakanese, and the like—and are now Burmese.

Chakravarti's study is excellent, drawing together a wealth of information for the student of Burmese and Indian affairs. Painstaking in its detail, gentle though sad in criticism of the schism that persists between alien and indigenous, it stresses the point that the Indian community throughout the period from 1852, the annexation of Lower Burma, to 1941 was only a small portion of the total population. The Indians at their height made up only 6.9 per cent of the total. Moreover, while portions of the Indian community prospered—as did the Burmese at the same time—major parts of this population remained desperately poor. The Indian coolie did the labor that Burmans would not undertake. "He suffered silently from long hours of hard work, scanty wages, rotten food

and wretched shelter." Rich and poor, the Indians were a highly visible alien people.

Chakravarti quite properly notes that from the start of British rule there was a significant Burman protest against the inflow of Indians. The young Burmese nationalists were speaking out on this issue. There was, however, very little intercommunal strife until the 1930s. As always, the problem came to a head at a time of economic privation when the continued unrestricted immigration of Indians was adding to the poverty and unemployment of those Indians already living in Burma. It is impressive, nonetheless, that all through the British era Indian immigrants rarely displaced Burmans from positions they held. In this area Chakravarti concludes that "if it was required to choose out of many evils the one which did greatest harm to Indian interests and contributed most to the unhappiness of Indians in Burma, the choice should unreservedly fall on the erroneous immigration policy, assiduously and obstinately followed by the Government of India, for decades, in the teeth of Burman opposition." Of course this was true. The unrestricted migration was sure to rouse resentment, particularly when the immigrant remained identifiable and aloof. A moneylender-banker is a moneylender-banker, and there were plenty of them among the Burmans. A Chetary moneylender-banker, however, is an alien.

Perhaps the problem of the Indian in Burma is best presented in Chakravarti's summary of a 1930 Legislative Council debate. An Indian member of the Council, who insisted that the Burman race and the Burman nation were not identical, protested when Burmese nationalists raised the spectre of the "Indian Menace." A Burmese nationalist replied that "we will be glad if you come to this side of the House and put on a *goung boung* and *paso* [Burmese dress] and call yourself Maung Ni. We will receive you with open arms."

The Indian minority in Burma would not then and still has not taken this advice. And in the period of intensely xenophobic nationalism that follows the ouster of an imperial ruler—even though the British left peaceably and voluntarily—this minority has been forced out of any meaningful role in Burma. Chakravarti's study is thus neatly and precisely confined. It is unlikely that the phenomenon of a steadfastly alien immigrant community will occur again, but in this work the reader has an excellent study of such a community's un-

questioned contributions, and its inevitable demise.

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DAVID JOEL STEINBERG *et al.* *In Search of Southeast Asia: A Modern History*. Edited by DAVID JOEL STEINBERG. New York: Praeger Publishers, 1971. Pp. xii, 522. \$12.95.

There is irony in the fact that *In Search of Southeast Asia* should have become available to students and teachers of Southeast Asian history at a time when this area of historical studies faces so many problems, particularly those of funding. This excellent textbook was published in 1971, but its conception dates from the middle sixties when the study of Southeast Asia seemed set on an uninterrupted and ever-upward path. By the time of publication the fallacy of a belief in progress was again revealed. Southeast Asian studies programs have been cut back, courses limited, and numbers of students curtailed. All of this has happened when, for the first time, a historical text became available that genuinely reflected the scholarly advances that had been made in the study of Southeast Asian history.

Yet if this situation is a matter for regret, no such feeling may be associated with the book itself. As the product of collaborative effort, the text is notable, not least, for the way in which six specialists have been able to produce a coherent and unified whole. Even from a stylistic point of view this unity is seldom drawn into question. Only an occasional flatness in the prose suggests some of the quite remarkable problems that were overcome in the course of the book's production.

Style, however, is of much less importance than the way this book provides an authoritative review of the major issues in Southeast Asian history from the eighteenth century to the mid-twentieth-century world. Few will agree with all the interpretations, and individual historians concerned with particular countries may find particular points of details where there are grounds for disagreement. Such areas for debate are to be expected in all but the most bland, and therefore unsatisfactory, of texts. In the case of this book the extent to which the joint authors have striven to take due account of all major questions of interpretation is the point requiring both stress and admiration.

The book is divided into five parts: "The Eighteenth-Century World," "New Challenges

to Old Authority," "Frameworks for Nations," "Social Changes and the Emergence of Nationalism," and "The Preoccupations of Independence." This arrangement, with its emphasis on conceptual analysis, does seem to assume that the reader already has some basic knowledge of the Southeast Asian region and its history. In this respect, and having used the book as a text for both undergraduate and graduate students, I am inclined to judge that its greatest value is for the already informed, if not necessarily advanced, student. In making such a judgment there should be no suggestion that this book is unsuitable for undergraduates. But *In Search of Southeast Asia* is, and this should be recognized, a sophisticated text. Offered to an uninformed audience it does require a certain commitment on the part of students, particularly a commitment to pursue the suggested additional readings that are amply and carefully selected.

Indeed the bibliographic service provided by the extended bibliography is a further important contribution made by this volume. One minor word of qualification might perhaps be entered here. The presence in the notes and bibliography of a substantial number of references to non-Western materials is an admirable reflection of the authors' linguistic capabilities. A student, on the other hand, might ask if some of these references could be deleted in favor of more readily available, if not necessarily more valuable, Western-language references.

This, however, is a minor matter, as is the further observation that one might expect the authors, when they come to consider changes for a second edition, to rethink, at least in part, some of the approaches followed in part 5 of the volume.

Given the book's value, even to end with minor criticisms is inappropriate. The publication of *In Search of Southeast Asia* may not mean that the search for well-written texts has ended. It certainly does mean that a new and very high standard of quality has been set, and by authors well-attuned to the vital issues of Southeast Asian historical scholarship.

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JEAN CHESNEAUX *et al.*, editors. *Tradition et révolution au Vietnam*. (Sociologie et tiers-monde.) Paris: Éditions Anthropos. 1971. Pp. 499.

In the quarter century following World War II

there appeared in a number of journals in France and occasionally elsewhere essays on Vietnam written by members of the Franco-Vietnamese community of Paris. These writers, quite varied in their approaches, took it upon themselves to educate the Western world on the reality of Vietnam. Jean Chesneaux and his students have drawn upon this scattered production to compile a volume that reflects the concerns of these specialists. Interpretive essays, scholarly pieces, journalism, and exposés, all exist among the eighteen presentations, forming a collection that provides a vivid pastiche of Vietnam in the modern age.

In showing how Vietnam has moved from the traditional situation to the present (as seen especially in the DRV) these authors have properly stressed the spirit of Vietnamese civilization in its resistance to the encroachments of the outside world. The result is successful in the sense of action that is given. Ideologically, politically, militarily, and economically, the dynamism of those seeking a free and independent Vietnam emerges as the main theme of its modern history, working toward an ultimate success. Ideology and tactics succeed only as they fit into the fundamental cultural pattern and creativity of the Vietnamese. Foreign manipulations fail because they create artificial situations and stimulate the inherent resistance of the Vietnamese.

Still the degree of success in this work must be qualified by the relative lack of penetration into the Vietnamese scene itself, the stage on which the action takes place. The descriptions of the indigenous patterns of belief are too general, and a much more comprehensive analysis of Vietnamese society is needed before we can gauge the full meaning of the variety of events presented here. The one piece that begins to give us this sense of depth is that on the great Vietnamese epic, *The Tale of Kieu*, where Chesneaux and Boudarel describe its political and social dimensions in the DRV of the 1960s.

By contrast, the strength of this piece reflects the weaknesses of the collection. Only three essays are based directly on research in Vietnamese language material. Vien has portrayed well the modern Vietnamese experience (including his own), the interviews by Lacouture, Devillers, and Dranber came directly from Vietnamese figures (Ho, Giap, etc.), Condominas and Dumont made their descriptions from first hand observation, but only Son and Boudarel have worked extensively in Vietnamese writings.

The remaining essays, many of them quite good, still draw upon Western documentation.

Overall, however, we can be glad that Chesneaux has made the effort to compile these pieces. The authors are men with a greater experience in Vietnam and a greater sensitivity for the Vietnamese than the American writers of the same period. By both its strengths and weaknesses this work shows how we must proceed in our own studies, and Chesneaux's initial statement can only be commended: Vietnam must be seen in Vietnamese terms. With this in mind, this collection can certainly be recommended as a good beginning for the study of Vietnam in the twentieth century.

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ANTONIO DE MORGÁ. *Sucesos de las Islas Filipinas*. Translated and edited by J. S. CUMMINS. (Works issued by the Hakluyt Society, Second Series, number 140.) New York: Cambridge University Press, for the Society. 1971. Pp. xi, 347. \$16.00.

Antonio de Morgá's *Sucesos*, originally published in 1609, was the first formal history of the Philippines and their conquest to be written by a layman. A high official of the government at Manila from 1595 to 1603, Morgá witnessed and even participated in many of the episodes he described, and he had access to survivors from the colony's earliest days. His account is a primary source of unusual value for the study of early Hispano-Philippine history. Previous editions—most notably those by H. E. J. Stanley (1868), José Rizal (1890), Blair and Robertson (1904), and W. E. Retana (1909)—have markedly influenced modern thought about the subject both by making available Morgá's information and also by using it as a foundation for the editors' moral and national interpretations.

The *Sucesos* is, in effect, two books: an episodic, disjointed narrative history of wars, intrigues, diplomacy, and evangelization and an essay (chapter 8) on the culture of the Filipinos, the resources of their islands, and the economics and institutions of Spanish rule. The first of these is of limited interest by now, although it is occasionally dramatic in the telling and suggests the role of clericalism, geographic remoteness, and strategic vulnerability in shaping the Spanish regime. The second is the heart of the work for modern readers. Morgá was a historical anthropologist of some subtlety. His coverage is impressive—Stanley discreetly left

untranslated a paragraph on sexual practice—and his perspective humane. Morgá took Indian culture seriously and recorded not only the skills, ethics, and social patterns that existed, but also those that had been lost or compromised under the impact of Spaniards and Chinese.

Given the paucity of sources for this period and subject, it was time for a new edition of Morgá that would be more accurate, more objective, and more readily available to students and new libraries. Cummins has met the need. His translation is lucid and pleasingly unobtrusive, and it is accompanied by a glossary, maps, erudite annotation, and a useful bibliography of modern scholarship. A careful introductory essay provides biographical perspective on Morgá and a historiographical discussion of the book in its earlier editions.

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PHILIP LOH FOOK SENG. *The Malay States, 1877–1895: Political Change and Social Policy*. (East Asian Historical Monographs.) New York: Oxford University Press. 1969. Pp. xvi, 233. \$9.75.

A large number of books and articles on modern Malay history has appeared in the last decade and a half. Such studies are limited by the nature of the source material available, and, therefore, to British activity both in the Straits Settlements as well as in British Malaya. One major focus of the scholarship of the early sixties was British intervention in the Malay States in 1874, which ended the policy of nonintervention of the previous half century. Professors C. Northcote Parkinson, John S. Galbraith, C. D. Cowan, David MacIntyre, Kim Khoo Kay, and myself examined the question from varying angles. Meanwhile, the University of Malaya at Singapore and Kuala Lumpur produced a number of bright young scholars whose interest, characteristically enough, was in the Malay states proper. The volume under review, a very sophisticated and extended version of a master's thesis, deals with principles underlying British policies, not imperial but local, in their application to southern Malay states before the latter were brought into a federation in 1895. The author's focus is not the Colonial Office in London, where policies could be assumed to have their origins, but British administrators in Malaya and their "attitudes and motives which had a part in determining both the political formation and the social framework" of the Federated Malay States—Perak, Selangor, Pahang, and Negri Sembilan.

Seng's study begins with 1877 in the aftermath of the Perak War, which was until World War II the only challenge to British authority in Malaya. The period before the arrival of Sir Frederick Weld in 1880 is treated as "transition years of political adjustment" during which time "the British commitment was to restore the form if not the substance of a Malay Sultanate" in each of the Malay states brought under the residential system. Seng has assumed that serious political thinking on crucial questions—such as how the Malay states should be governed, what the nature of the relationship between the ruler and the ruled should be, and other related issues—was undertaken for the first time by Weld. Much of the work is consequently devoted to Weld's term as governor.

However important Weld's governorship was, and it was important, residents like Hugh Low and his able assistant William Maxwell had laid down principles of administration in crucial areas like land ownership before Weld's arrival. In fact, among the best sections of this book is the one dealing with land policies. In Perak Low's numerous policy ends included the maximum exploitation of tin mines for increasing state revenues and also for permitting the introduction of British capital in mines already owned by the Chinese. This was achieved through a 1879 regulation that "no land should be allowed to lie idle if anyone is willing to work it, and the owners of metalliferous land must submit to its being worked" (p. 112), as long as the government royalty was paid. A proliferation of fresh European concessions also came about as lands for "agricultural industries" were granted to European concessionaires, often friends of the resident (p. 118). Such a policy of favoring British capital became much more evident during the governorship of Weld, an archimperialist, during the period of the so-called "economic imperialism."

In bringing out Weld's thoughts on administration Seng has perhaps paid greater attention to the clash of personalities—governor and residents and Colonial Office—than is warranted. It would instead have been more instructive in terms of the author's self-chosen task of studying the interaction between political change and social policy to afford some insights into the dynamics of local politics and the changes brought by the introduction of modern economic devices and technology. Hardly any consideration is given to such issues because they are "but faintly perceived in the corpus of English documentation utilized here," thus underlining the serious limitations on

scholarship in modern Malay history alluded to earlier.

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H. C. BROOKFIELD. *Colonialism, Development and Independence: The Case of the Melanesian Islands in the South Pacific*. New York: Cambridge University Press. 1972. Pp. xvi, 226. \$16.50.

A distinguished geographer with much experience writing about Pacific island areas, especially New Guinea, discusses here the economic history of Melanesia by means of a conceptual frame. He centers most of his attention on the period since 1945 and skillfully demonstrates the widely differing economic development of various parts of the area.

This is a difficult book for a historian to evaluate. It lacks notes, it abounds in social science jargon, and it attempts to build a model applicable to studying the development of heterogeneous peoples scattered over a wide area of the Pacific and whose dissimilarities presumably outnumber their similarities. On the other hand the author makes sound economic judgments, demonstrating a thorough if not intimate knowledge of the area, and presents valuable economic evidence to support his thesis.

Professor Brookfield begins by declaring that he writes "not so much as a geographer, but rather as a social scientist who is also an observer of contemporary trends" (p. x). This may be valuable because of his background and experience, but when he turns to his model, that is another story. He uses the terms "colonialism" and "independence" in a much broader than political sense to explain "opposing drives which persist throughout the whole period of interaction between local forces and invading forces from without" (p. xi). Not only are there inherent problems in attempting to define these terms, which the author concedes, but the definitions do not begin to fit the range of possibilities. For example, the roles of culture and of individual behavior are mentioned, but they are not considered important enough to contribute substantially to the operation of the model.

In addition, some of the areas surveyed, such as the Solomon Islands and West New Guinea, do not fit the system during long periods of their recent history. Further, in discussing the problem of pluralism in Melanesia—and what

a problem it is!—the author admits that to fit it into his model becomes “a gross simplification” (p. 146). One better alternative to such an approach is individual histories of topics or areas that represent cohesive studies that are valuable by themselves but that may be used together. For example, Dorothy Shineberg’s study of the sandalwood trade between 1830 and 1865 and R. A. Derrick’s history of the Fiji Islands to the 1940s are both more pertinent than this work because they combine sound methodological technique with competent evaluation of a limited subject. Many of the social scientist’s techniques may be valuable to the historian, but in this case the generalizations and the continual oversimplifying make this book too uneven and too unreliable to induce the reader to spend the time necessary to mine its buried nuggets of economic data.

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UNITED STATES

JOHN E. POMFRET. *Colonial New Jersey: A History.* (A History of the American Colonies.) New York: Charles Scribner’s Sons. 1973. Pp. xix, 327. \$10.00.

This first volume of a projected series of thirteen dealing with the original colonies is not completely free from the narrow provincialism often associated with state histories. Eight of the twelve chapter headings in this study of colonial and Revolutionary New Jersey indicate an emphasis on political history, and actual perusal of the contents reveal an even greater commitment to narrative political and religious history with little actual attempt at the interpretation promised on the dust jacket.

Dr. Pomfret’s text substantiates his contention that New Jersey’s bipolarity and lack of a sense of identity contribute to the complexities of the narration, but this is no excuse for inadequacies such as the sketchy and dated material on the Indians and on the pre-1664 colonies or the social and economic backdrop limited to furtive statistics and almost painful platitudes. The politically derived, detailed coverage of colonial currency and exchange problems illustrates an imprecision in the handling of terms such as fiat money, legal tender, land banks and loan offices, and a lack of knowledge of recent scholarship in areas of paper money and the Currency Act of 1764. The statements that New Jersey’s £347,000 paper money emission was colonial

America’s largest and that New Jersey paper passed at a premium (actually at par) are easily checked inaccuracies as is the omission of mention of the mother country’s 1774 approval of a New Jersey land bank.

In a work with such narrative detail, the omission of many New Jersey personalities such as Peter Hasenclever, Caspar Wistar, and Mollie Pitcher is startling. The ideological significance of New Jersey as the battleground of Old Lights and New Lights and the related dynamics of college founding are largely glossed over or ignored in favor of interminable tenure listings of ministers.

The bibliography reveals an unfortunate emphasis on traditional and regional works and few of those recent interpretative works dealing with class, caste, institution, power, and ideology that are remolding our understanding of colonial America. It should be possible, even when seeking a general audience, to enliven such a work with current conceptualizations without getting involved with the obfuscations of some contemporary methodologists. The restatement of the two revolutions theme of Becker and Jamison and the judgment that New Jersey’s revolution was conservative and consensual seem to be an afterthought and are less than convincing given the limited evidence.

This volume better meets the needs of neophyte students and the general public than does any other existing single volume, but a definitive interpretative history of colonial New Jersey is still wanting.

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BRUCE E. STEINER. *Samuel Seabury, 1729–1796: A Study in the High Church Tradition.* [Athens:] Ohio University Press. 1971. Pp. xiii, 508. \$13.50.

This is a strange book. Dr. Steiner has stirred together a lumpy brew, mixing extremely important and needed narrative history of the early years of the Protestant Episcopal Church in America, biographical data of Bishop Samuel Seabury, and trivia strained fine indeed. We have long needed a thorough study of the life of Seabury, and this work, in part, fills the void. The best portions of the book explore Seabury’s failings as a parish priest (which will be an eye-opener for many Episcopalians), his prominent role in the pamphlet war over King’s College, the controversy over the authorship of the A. W. Farmer essays,

and Seabury's appointment as bishop by the Episcopal Church of Scotland after he had been refused by the Church of England.

Yet the author never seems clear as to which part of his material is important and which is not. Thus the reader encounters page after page of information about parishes grown more constant and devout. We are told of the graduation dates from college for nearly every character who appears in the text (William White [College of Philadelphia, 1765], the Reverend Bela Hubbard [Yale, 1758], William Smith [Aberdeen, 1774], *et cetera ad nauseum*). We learn such potent facts as that late in 1742 Robert Jenney, who had for many years been minister at Hempstead, accepted a call to Christ Church in Philadelphia. Naturally enough we learn that Jenney graduated from Dublin in 1709. Surely much of this information is nonessential and some could be of interest only to the most antiquated of antiquarians.

The author's style lacks both grace and clarity. Lengthy quotes are frequently included, too easily passed over and forgotten. Finding a page without several parenthetical expressions is a monumental occurrence. Obscure terms are used repeatedly without definition, a small difficulty perhaps, but it would have been easy for the author to tell his readers that "glebe," for example, was cultivable land owned by a parish or benefice, the income from which was used primarily for the support of a cleric. An early and continuing difficulty in Steiner's book arises from the plethora of Seabury's named "Samuel" with which he deals: a great-grandfather, a great-uncle, a father, a son, and a grandson of Bishop Samuel all bore the same Christian name. The author's style makes it difficult at times to know which man he is referring to.

It is unfortunate that Dr. Steiner chose to refer to the Congregationalists as "Presbyterians," which they surely were not. As they themselves consistently and continually made clear, the polity of each group was very different. One ponders the reasons for Steiner's choice in this matter. Furthermore, as a work subtitled *A Study in the High Church Tradition*, it makes little attempt to assess the theology, ethics, metaphysics, dogmatics, or Christology of either Seabury or the Episcopal Church. Of ecclesiology we are given a little, and in one chapter—the last save the epilogue—we are introduced to Seabury's view of the Eucharist. Surely more space could have been devoted to such topics and a little less space

given over to informing us of graduation dates.

I have written of disappointments in the book, yet it has many good qualities. It contains everything anyone could possibly desire to know about Seabury and his daily activities, including a great deal of unfamiliar material that will add considerably to our knowledge of the man. It is based upon the solidest kind of research, and supernumeraries constantly remind the reader of the sources from which the material has been drawn. The notes, placed in the rear, cover almost one hundred pages! The bibliography is both extensive and inclusive, and the index leaves out no important name or topic treated in the text that I could discover.

CECIL B. CURREY

University of South Florida

DIRK HOERDER. *Society and Government 1760–1780: The Power Structure in Massachusetts Townships*. Berlin: John F. Kennedy-Institut, Freie Universität Berlin, 1972. Pp. ii, 200.

We can learn much from Dirk Hoerder's structural description of five Massachusetts towns. For the towns themselves—Amherst, Weston, Worcester, Plymouth, and Boston—Hoerder supplies some new data and, more importantly, a restrained statistical framework that allows genuine comparisons. Half the study is devoted to a detailed set of appendixes and a remarkable bibliography.

By beginning his description with Amherst (whose very newness blurred the expected correspondence between status, economic well-being, and political expectation) and then moving along a continuum of increasing age, size, and economic complexity Hoerder argues that colonial Massachusetts's egalitarianism was more the product of incompleteness than any radical redefinition of the social order. Settlements, which grew and survived, readopted reigning notions of social deference just as soon as their size and internal complexity allowed the identification of a coherent group of social notables. Once in power, these men looked after their family interest with all the tenacity one expects of a self-important middling gentry.

Hoerder's failings are equally instructive for they mirror—and occasionally caricature—our present preoccupations. Trained at both the University of Minnesota and Berlin's Free University, Hoerder's theoretical perspective is a curious amalgam of the European's notion of social class and structure and the American behavioral scientist's concern with political

power and community typologies. Applied in a historical setting such a perspective leads Hoerder—just as it has so often led the rest of us—down blind alleys and into meandering diversions. Unfortunately our language for social analysis still lacks the suppleness a creative melding of history and numbers requires.

ROBERT ZEMSKY
University of Pennsylvania

MARVIN KALB and ELIE ABEL. *Roots of Involvement: The U.S. in Asia, 1784-1971*. New York: W. W. Norton and Company. 1971. Pp. 336. \$8.95.

Marvin Kalb is a diplomatic correspondent for CBS News in Washington; Elie Abel, once the same for NBC News, is now the dean of the Graduate School of Journalism at Columbia. They are judicious analysts and skilled writers, and they have put together a book that will provide the general reader conceptual continuity in making sense of the Vietnam trauma. In so doing they offer future historians much detail based on interview that will help illuminate the documentary mass already available, as well as the mountains that will some day be declassified. Instead of *Roots of Involvement: The U.S. in Asia 1784-1971*, however, the professional historian might wish to call it *Vines of Entanglement: The U.S. in Indochina, 1945-1970*. The title promises more than the book delivers.

The authors do not consider root causes closely. They do assemble a plausible narrative toward demonstrating that "the inexorable progression—from Yankee clipper to Yangtze gunboat to helicopter gunship—suggests that Vietnam was but a terrible moment in America's swashbuckling adventures in Asia." The book, however, is much better than the sentence quoted. An excellent work of narrative journalism, it soundly concludes that "even the most powerful of nations can exhaust and discredit themselves by overspending their resources in a dubious cause."

The book is not footnoted, nor are sources listed. The authors appear to be familiar with the most revealing of books with a Washington focus by participants and observers through 1970, but the Pentagon Papers were published at about the same time as their own book. They have not consulted works with primary focus on modern Vietnamese history, such as McAlister's *The Vietnamese and Their Revolution* (1970). The strength of the work of Kalb

and Abel lies in their accumulated knowledge as Washington correspondents, their two dozen acknowledged interviews with high-ranking officials, and other interviews with persons preferring anonymity.

What do they say? They say that Truman should have followed Roosevelt's lead in Indochina policy rather than Acheson's policy of putting European allies first, with its corollary of support of neo-colonial ventures. Eisenhower let Dulles draw a moralistic line against communism that led to a commitment to Vietnam—Eisenhower's Gettysburg speech of 1959—in the name of national interest. Kennedy tragically let slip the opportunity to take a critical new look at the involvement in Vietnam. Johnson, with a gravely narrowed list of options, plunged unreflectively into the most dangerous of them, and only by withdrawing from national life was he able to begin to reduce an "Asian obsession" to a "nuisance." The book was apparently finished some months after the Cambodian incursion and nearly two years before the Kissinger-Tho agreement; it ends on a cautiously trustful note that Nixon would continue to "winch down" the war. In my opinion—an opinion obviously subject to significant change as time proceeds and documentation accumulates—the book undervalues Eisenhower's conditioned refusal to engage in overt military support of France in Indochina in 1954, and overestimates the value of his Gettysburg speech. More important, it underestimates the gravity of the steps that Kennedy took in 1961 in response to the report from Taylor and Rostow.

There are, I believe, some points in this history when a different decision would have made a great difference. The authors are sensible enough to believe so, too, as their thoughtful handling of particulars frequently shows. Nonetheless, by insisting that Vietnam was not an "aberration" but a logical consequence of long-held American attitudes and assumptions, they proceed greatly to overstate the "inexorability" of American entanglement. They do not accept the assumption that the war was waged for motives of economic imperialism, and the lack of putative or real economic benefits supports their argument. But by speaking of romance and self-righteousness going back to the voyage of the *Empress of China* in 1784, they do not present a compelling case for inevitability.

A series of allusive connections woven into a narrative of disaster does not constitute the inexorable. George Ball was correct when he

cautioned President Kennedy that by agreeing to the small increases in manpower asked by Taylor and Rostow in 1961 he would have to go to 300,000 men in a few years. Further increases took the commitment beyond half a million men. In 1968 Clark Clifford grew apprehensive that "someone is going to want to round it off at a million." The change came: a slow winching down of what had been slowly cranked up in the seven years previous.

Mechanical metaphor, however, is weak. This is a story of how some illusions became obsessions, and how the obsessions became intolerable to the people who held them. Some day there will be enough perspective on the story for it to be told as a major tragedy of American history, and of the Vietnamese, Cambodian, and Laotian histories upon which American actions impacted.

THEODORE FRIEND
Swarthmore College

RICHARD R. BEEMAN. *The Old Dominion and the New Nation, 1788-1801*. [Lexington:] University Press of Kentucky. 1972. Pp. xiv, 282. \$11.00.

This book would have made a good, longish article or perhaps even a monograph of fifty or seventy-five pages had the new and useful information in it been separated from the considerable space devoted to surveys of foreign affairs (as in discussions of Jay's Treaty and Franco-American relations in 1798), oversimplified accounts of national politics (Hamilton's financial policies, for example), and misunderstandings of theoretical issues (such as efforts to extend religious liberty in Virginia). Beeman shows us in illuminating detail how the Virginia press reacted to national issues during the period 1788-1801, how public meetings in many counties were used by both Republicans and Federalists to clarify issues and to consolidate support, and how the traditional system of control by "leading families" remained substantially in effect within both parties in local and state government. Such a descriptive monograph would have made available data valuable to subsequent studies of the politics of the Federalist era.

As it stands, though, the book is simply too often dubious or misleading in its interpretations. In "proving," for example, that James Madison switched from being a nationalist before 1790 to being a Virginia parochial after that date, in order to retain his power in the traditional state political structure, Beeman

fails to mention Madison's two years as a member of the Virginia Governor's Council and his assiduous attention to state problems during his years in the Continental Congress—a stance abundantly clear in recently published volumes of *The Papers of James Madison*. Just as injudicious is the author's assertion that Madison's opposition to Hamilton's funding and assumption plans rested on the Virginian's state bias. Madison's position was indeed mindful of state interests, but his statesmanship was to see how these local needs were consistent with a plausible, attractive, perhaps more noble conception of national growth than that of the secretary of the treasury. Furthermore, Beeman's argument that Virginia anti-Federalists opposed grand titles for the president in 1789, entirely overlooks Richard Henry Lee's firm support for them in the Senate, another example of the author's distressing tendency to ignore facts unsuited to his thesis.

The major misconception marring the book, though, is that Jeffersonian Republicanism was basically hypocritical because it did not repudiate the traditional political system in Virginia, extend the franchise, free the slaves, establish a public school system, and virtually dismantle the federal government. Since no one in Virginia during the 1790s (and certainly not John Taylor of Caroline whom Beeman often uses as an example of a "radical" Republican) advocated such a program, it is very easy, of course, to "prove" that the Jeffersonians did not! Hence the author incessantly topples straw men and puts down the Jeffersonians for not being 100 years ahead of their time. Unfortunately such unfruitful analysis largely submerges the useful information Beeman has gathered about reaction in Virginia to national political issues.

RALPH KETCHAM
Syracuse University

NATHAN REINGOLD *et al.*, editors. *The Papers of Joseph Henry*. Volume 1, *December 1797-October 1832: The Albany Years*. Washington: Smithsonian Institution Press; distrib. by George Braziller, New York. 1972. Pp. xxxix, 496. \$15.00.

The name of Joseph Henry is well known to many American scientists because of his association with the Smithsonian Institution (of which he was the inaugural secretary) and because the unit of self-inductance is called the "henry" in his honor. But he is a shadowy

figure, even for historians of physics who may not have a special interest in American science. A central problem is that while Henry was unquestionably the foremost American physicist in the years between Benjamin Franklin and J. Willard Gibbs, it is difficult to determine just how good he was. Credited with some major experiments and concepts in electromagnetism he was all but eclipsed by the great Faraday, and he never achieved the eminence of being elected a foreign fellow of the Royal Society or a foreign associate of the French Academy of Sciences.

In mid-career Henry gave up an active life as professor at Princeton and vigorous experimenter to serve the larger community of science and the nation as secretary of the Smithsonian Institution. To this day the Smithsonian follows in broad outline the series of activities Henry conceived to be its mission, in terms of James Smithson's will: "to increase and diffuse knowledge." The introduction of the first volume of Henry's papers sees him also as "a founder of the American scientific community."

A major event in Henry's career was his appointment to Princeton, in 1825, the end point of the present volume. At that time he had achieved a significant position in Albany and was well launched on his career in experimental electricity. There exists very little manuscript material on Henry's early life, education, or family background. The editors have decided that, "aside from documenting Henry's personal career, a major task of this volume is to describe the Albany milieu as an influence on Henry's scientific growth." A footnote refers to "a brief family history as told by Henry's aunt, Elizabeth Selkirk," which contains Henry's own "notes on Elizabeth's account," but this document is not printed. Its omission evidently represents an editorial point of view concerning the significance of a scientist's family and ancestry. This conclusion is substantiated by the editors' own declaration of the "working resolutions" by which the volume was put together: "The first is to document Henry's research and professional career for an understanding of both science and the growth of the national scientific community. The second is to use the life of Joseph Henry as an occasion to present a broad documentary history of a period and a place, not merely a narrow recital of events in a career. The life becomes the thread upon which the beads of history are strung." Thus the aim of the editors is to demonstrate that

science is "integrally part" of "the national culture" and to achieve the goal of merging "imperceptibly the scientific work and the general background," and thus "to present few sharp edges between the 'internal' life of science and the 'external' milieu." In any event there appears to be very little surviving personal documentation of Henry's personal life or even experiments; accordingly, there is "very little to say about Henry before his twenty-fifth year." Again, because "so few manuscripts survive," Henry's "Albany work on electromagnetic induction is barely touched upon."

The first volume of these *Papers* is a monument of editorial industry. The editors have gathered together a mass of documentary information concerning the scientific, cultural, and social life of Henry's Albany: concentrating on two institutions with which he was associated, the Albany Lyceum of Natural History, of which he was one of the curators, and the Albany Institute. Albany in those days was the eighth or ninth largest city in the United States, a major center for state government, industry and finance, and culture. While we may regret the lack of information concerning Henry's own education we must applaud the editors' assemblage of documentary information on so many aspects of the cultural life of the Albany community of his intellectual formation, especially the sequence of public scientific lectures that he might have attended. Indeed, Henry apart, the editors have provided a model documentary history of the intellectual life of a major American city in the second and third decades of the nineteenth century that—*inter alia*—shows the great extent of interest in science. Thus this volume should become a primary source for cultural historians.

The editors are especially to be congratulated on thus reconstructing the background of Henry and his education for the years in which no documentation is available. Indeed, Henry himself comes alive in these pages only in his early maturity, with his correspondence with Harriet Alexander, whom he married in 1830, and with his notes and correspondence on scientific matters in the years 1830–32. These concluding documents of Henry's Albany period whet the scholarly appetite for the succeeding volumes.

The level of scholarly editorship of this volume is exceptionally high—not only in finding documents but in presenting them with a full panoply of editorial comment and ex-

planation (or amplification). There are very few readers, if any, even specialists, who will not find much that is new in these pages. It should be added that the volume is beautifully printed, in a manner worthy of the subject, and is reasonably priced.

I. BERNARD COHEN
Harvard University

FRANCIS N. STITES. *Private Interest & Public Gain: The Dartmouth College Case, 1819*. Amherst: University of Massachusetts Press. 1972. Pp. 176. \$9.50.

STANLEY I. KUTLER. *Privilege and Creative Destruction: The Charles River Bridge Case*. Philadelphia: J. B. Lippincott Company. 1971. Pp. 191. Cloth \$5.95, paper \$2.95.

Stites offers a case analysis of the famous *Dartmouth College v. Woodward* decision, which extended the contract clause of the Constitution to charters of corporations. Making few promises, and regrettably keeping his word, he states that the purpose of his study is simply to pull together the abundant material on the case that we already have and present it as a composite whole. The results of his research are even more disappointing than expected. The author fails to add measurably to our understanding of the decision and, what is worse, falls woefully short of the modest goal he has set for himself.

Drawing principally from the records and correspondence contained in the Dartmouth College Archives, Stites tells us more than we want to know about the tactics of the rival factions at the college and of the lawyers involved in the litigation; at the same time, the reader is left wondering whether anything worthy of mention took place beyond college walls. The author's research concerning events at the college is extensive and at times fastidious; but he scrupulously avoids such basic sources as newspapers of the day and most of the primary and secondary materials on Justice Story, a principal figure in the case; the concurring opinions of Justices Story and Washington are likewise passed over in his superficial analysis of the decision itself. He merely alludes to the decision's beneficial impact on business corporations and private colleges, missing his last opportunity to redeem the book. As a consequence of these and countless other errors and omissions, the author is unable to place the decision in its proper setting or relate it clearly to the political, legal, and cul-

tural history of the country. What we have here, at best, is a fatiguing article masquerading as a book. Happy is the library that did not purchase this cheaply printed, overpriced monograph.

Turning to Kutler's balanced and readable study of the decision that qualified the Dartmouth ruling, we leave the marshes, reaching high ground. "The Charles River Bridge case," he perceptively observes, "may well mark the origins of dissent within the Supreme Court." Mindful of the larger issues, Kutler paints a rich background, depicting the pecuniary interests at stake before the decision was handed down; he then completes the picture by showing the effects of the decision on American legal and economic history. The importance of the case stems from its legal rationale justifying "creative destruction"—that process whereby new enterprises and inventions create new goods and services and destroy existing ones in the name of progress. "It is this aspect of the case that transcends the relatively limited conflict over bridges in the Boston area, making it a landmark involving the interrelationship of public policy, technological change, capital development, competition, and law."

But "creative destruction" is not a legal rationale Kutler applauds in his final evaluation of the merits of the decision. Donning the hat of the early nineteenth-century conservative who criticized the application of the Utilitarian ethic to capitalism, the author indulges the "privileged" proprietors of the old bridge and their spokesman on the Court, Judge Story. Standing behind Tocqueville, who stressed the need of protecting economic minorities, Kutler condemns the expediency of the Massachusetts legislature; for when it authorized the new, free bridge and refused to compensate adequately the proprietors of the old one, it violated "in the noble name of 'community rights'" the "rights of others within that community." And it may be true that Taney's decision supported the public interest, but this interest should not always be determined by counting heads. "Surely," he concludes, "it is not always just—or even useful—to rescue that interest from the altar of privilege, only to sacrifice it in a holocaust to immediate popularity."

JAMES MCCLELLAN
Hampden-Sydney College

RONALD P. FORMISANO. *The Birth of Mass Political Parties: Michigan, 1827-1861*. Prince-

ton: Princeton University Press. 1971. Pp. xii, 356. \$12.50.

MARVIN E. GETTLEMAN. *The Dorr Rebellion: A Study in American Radicalism: 1833-1849*. New York: Random House. 1973. Pp. xxii, 257. \$8.95.

Since the pioneering work of Lee Benson on New York (1961) and William W. Freehling on South Carolina (1965), microcosmic studies have provided a montage of conflicting conclusions about Jacksonian America. These two books add to this growing corpus.

Ronald P. Formisano is Benson's most impressive student. Going far beyond his mentor in the utilization of quantitative techniques and in the sophistication of analysis, his case study of Michigan party development will remain a landmark in the historiography of this "era of the common man." Using election returns and contemporary testimony as evidence, as well as expanding upon Alexandra McCoy's excellent analysis of elite behavior, and testing all this with analytical techniques borrowed from the behavioral sciences, Formisano argues that political controversy in the Wolverine State revolved not around socio-economic divisions but rather around ethnocultural ones. Whatever his weaknesses (a plodding literary style, difficult organization, occasional inconsistencies, and excessive repetition are among them), Formisano lays bare the problems of an economic conflict interpretation of early American politics. Far more important than economic status in determining political preference was region of birth (the Yankee versus Yorker battle continued on the western end of Lake Erie), religious preference (nonevangelical Catholics, Episcopalians, Methodists, and Lutherans versus evangelical Presbyterians, Congregationalists, and Baptists), and national origin (native whites versus immigrants). But in destroying the class conflict thesis, he does not adopt a consensual school theme of political infighting merely for the fruits of office. There were differences between Whigs and Democrats. The former's evangelical tradition considered partisanship to be secular and profane and allowed individual conscience to determine party allegiance, while at the same time Whigs sought "an organic society with specific ethnocultural and religious traditions" (p. 127) achieved through governmental enactment and enforcement. The democracy's adherents valued individualism over governmental control of ethical standards and supported a tolerant attitude regarding the im-

migrants' role in politics while subordinating individuality to party regularity. As long as the Protestant majority remained divided over various issues, the Democrats normally won.

A series of events combined to destroy the Democratic monopoly during the 1850s. The rapid increase in foreign, largely Catholic, immigration, the entrance of the Catholic hierarchy and Mormons into politics, a revulsion at Southern dominance of national politics, a growing intransigence relative to such issues as slavery extension, temperance, public education, and voting registration, and an animosity toward conventional politics and politicians all contributed to political independency and support of the Republican party. Particularly significant was the shift by the Methodists and New British immigrants into the evangelical, Republican camp. While this is a somewhat captious criticism, it would have been interesting if Formisano had correlated mass voting behavior with legislative voting behavior, which could add significantly to our understanding of partisan solidarity and the persistence of antipartyism.

While Formisano makes significant additions both to the understanding of Jacksonian politics and to historical methodology, Marvin E. Gettleman's analysis of the quest for broadened suffrage in Rhode Island does neither. One wonders if the kind of analytical techniques used by Formisano would contradict or confirm the geographic-economic cleavage Gettleman portrays. When Gettleman tries to use Robert Merton's "The Self-Confirming Prophecy" to support a contention, it is stuck in a footnote and not integrated into substantive argumentation. The failure to use such paradigms as Harry Eckstein's on internal wars limits the value of this monograph. Moreover, because of the author's sympathy with Thomas Wilson Dorr (1805-54) and his most avid followers, he fails to acknowledge that the Suffragists secured relief from their major dissatisfactions with the Charter of 1663 upon the approval of the "Algerine" Constitution of 1845. Despite his attempt to show how "the nation's revolutionary beginnings transmitted an undercurrent of genuine radicalism" to "antebellum American political life" (p. xx), it is equally apparent that the same revolutionary tradition provided intellectual sustenance to the charter's supporters. And this "tradition" with its resort to violence and justification by popular sovereignty appeared elsewhere in Jacksonian America—the nativist riots of Eastern cities, antiabolitionist mobs, California's vigilante

groups, bleeding Kansas, and the anti-Mormon riots. Any tie to the Revolution is unproven. Finally, in discussing the external causes of the Dorrites' failure, Gettleman omits an obvious conclusion of his own evidence—timely concessions by the opposition. Though his writing style is lucid and felicitous, it is filled with loaded words such as “cruel,” “brutally,” “impassioned,” “passionate,” “haughtily,” and “learned.” Thus, while providing the best available description of the Rhode Island affair, *The Dorr Rebellion* makes little substantive contribution to our understanding of Jacksonian America or of any radical tradition.

DAVID CURTIS SKAGGS

Bowling Green State University

ROBERT J. PARKS. *Democracy's Railroads: Public Enterprise in Jacksonian Michigan*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1972. Pp. 261. \$12.50.

Until the seminal works of Louis B. Hartz and Oscar Handlin appeared nearly three decades ago, historians, often conditioned by an ethic of private enterprise and lacking tools of economic analyses, gave short shrift to the role that state governments played in the nineteenth century in the development of transport facilities and to the economic meaning of these and other forms of social overhead capital. Now scholars are gradually constructing a body of literature giving a new account to governmental enterprise and economic progress. In this study, an outgrowth of a dissertation based primarily on public records, Robert Parks effectively narrates and analyzes the ways in which the territorial and state governments of Michigan, usually dominated by Jacksonian Democrats, planned, built, and operated railroads in the 1830s and 1840s.

Seeing the ideology of progress, the involvement of the national government in the promotion of internal improvements, and the national credit structure as external forces at once supporting and inhibiting railroad projects in Michigan, Parks asserts that essentially a growing population, its fortunes dependent on the marketing of agricultural produce, prevailed in a call for the state, which it saw as an agency to protect the people against monopoly, to build a publicly owned railroad system. Local interests, appearing to compose sectional groupings, then compelled the state legislature to approve the construction of three main railroads, the Southern, Central, and

Northern roads, and ancillary railroads and canals. Unfortunately expenditures for multiple projects deprived the one really viable railroad, the Central road, of funds for more motive power and rolling stock. Indeed, even though the state managed the Central road reasonably well and realized profits from its operation, the state legislature finally decided to sell it to private interests rather than increase the public debt or taxes to finance a rebuilding that might have enhanced the road's profits. Neither the Southern nor the Northern lines, the author concludes after a careful financial analysis, met the test of profitability.

Looking at the broader economic import of the state railroad system Parks finds that the Central and Southern railroads had “Smithian” effects in promoting agricultural specialization in the areas they traversed. He ascribes only nominal importance to the railroads in generating manufacturing linkages. Arguing that the Central and Southern roads ran through sections where the density of population in the 1830s could yield satisfactory returns he challenges Albert Fishlow's assumption that these railroads were constructed ahead of demand. More stridently he dismisses Robert Fogel's contrafactual proposal for a canal system in Michigan as an adventure in abstraction. Rather, insists Parks, one single railroad from Detroit to St. Joseph or Chicago, with appropriate feeder lines, would have more productively served the state. Some state legislators saw the wisdom of such a project, but economic realities were not compatible with representative democracy.

Unquestionably, this study is a solid contribution to the literature bearing on the relationship between state governments and economic development. If there is some general criticism to offer here it is that owing in part to his compartmentalization of economic and political issues, Parks does not sufficiently tie political ideology to economic decisions, especially in respect to the sale of the Central railroad. The title of the book is a bit misleading: readers without a specialized knowledge of the political terminology of the period may assume that the author is referring to democracy in a generic sense when, of course, he is using the old denomination of the Democratic party. He is not convincing, incidentally, in portraying the initiation of the state system of railroads as a special triumph of the Democrats and indeed acknowledges that the Whigs were not opposed to the initial program for the system. Historians not well

versed in economic analyses, particularly with the concepts of external economies and the technical indivisibility or "lumpiness" of social overhead capital, may occasionally struggle for understanding, but they should find the effort rewarding.

CARL M. BECKER
Wright State University

WILLIAM GERALD SHADE. *Banks or No Banks: The Money Issue in Western Politics, 1832-1865*. Detroit: Wayne State University Press. 1972. Pp. 328. \$15.95.

This is a detailed study of the bank issue in the states of the Old Northwest—Ohio, Indiana, Illinois, Michigan, and Wisconsin—in the thirty-odd years from Andrew Jackson's "Bank War" to Appomattox. Using impressive research as well as quantitative analyses of voting in the legislatures, constitutional conventions, and referenda to the electorate, the author seeks to add a new perspective and to prove the major thesis that the antebellum fight over banks was not "simply a conflict between the haves and the have-nots," but "an aspect of the broader conflict between political subcultures that structured partisan controversy within the area" (p. 18).

The book traces the shaping of attitudes of two opposing coalitions. "Agrarian-minded" Democrats, who by the mid-1840s dominated the party's thinking, moved from the exoneration of Old Hickory and the blame of the banking system for the coming of the panic and depression of 1837 to a "hard" position of opposition to all banks. Banks corrupted the legislative process and violated the Constitution, equal rights, historical experience, and "true philosophical principles." To this group—a heterogeneous compound of Germans, Irish Catholics, and upland Southerners within the Democrat party—banks became a symbol of and a focal point for resistance to an increasing commercialization of American life and culture fostered by Yankee-Protestant Whigs and after them Republicans, who came to regard free banks under general incorporation laws with notes based on government paper as tools vital to economic development and general prosperity. These two antagonistic points of view would be sustained in bitter debates in constitutional conventions in all five states between 1846 and 1851, in arguments over free-banking legislation proposed (and adopted) by four of the states in 1851 and 1852, and again, in discussions after the fiscal debacle of 1857, and especially during the political crisis of

1860-61, out of which national banking laws would ultimately come.

This is a thoughtful, carefully executed book. Yet its main thesis is at times fragile and elusive. Thought and rhetoric on the bank question are not always separable. Factor analysis is not completely reassuring, and one has to wonder why all vote maps and tables are concerned only with Illinois. At the same time one must admire *Banks or No Banks* as a serious attempt to view both the real and symbolic dimensions of an issue in the broadest social context.

CLARK C. SPENCE
University of Illinois,
Urbana-Champaign

ALAN DOWTY. *The Limits of American Isolation: The United States and the Crimean War*. Foreword by HANS MORGENTHAU. New York: New York University Press. 1971. Pp. xii, 272. \$9.50.

Alan Dowty distinguishes between American isolation during the nineteenth century and the isolationism of the 1920s. In the 1800s United States policy toward Europe combined neutrality with selective probing for political advantages. The interwar twentieth-century imitation was merely a passive aloofness unworthy of a great nation.

To document this thesis Dowty has minutely traced the diplomacy of the Franklin Pierce administration, which he considers a representative model. The essence of his interpretation is that President Pierce, Secretary of State William L. Marcy, and their diplomats perceived the Crimean War as a crisis that would draw European attention from the Western Hemisphere. The United States then could seize the opportunity to expand politically into the Caribbean, Central America, and Hawaii. These goals were not realized because the president and his State Department indecisively vacillated between bluff and the use of force, never successfully blending the two into an effectively expansionistic compound.

The theme that Europe's disputes work to America's advantages is not new, but Dowty has added the important qualification that at least one administration failed to gain from a major European political and military conflict. A quite different conclusion can, however, be reached from the evidence presented in this book. Passage after passage of quoted diplomatic dispatches to the State Department assert that the primary purpose of American statecraft is support of overseas American commerce. Marcy's man in Moscow, Thomas H.

Seymour, succinctly warned of the economic significance of the Crimean War. American commerce had "recently outstripped that of every other nation," Seymour wrote. "It would not be strange, if the continuance of a war, which manifestly is to be carried on for the purpose of destroying the trade of Russia by sea, and the destruction of her sea-port towns, should seriously affect us." In other words the Crimean War was yet another test of the viability of America's policy of neutrality, a strategem designed to maximize profit from oceanic trade during European wars.

Dowty admits that his documents demonstrate a keen interest in American foreign trade within the State Department and among diplomats, but he insists this concern was secondary to thoughts of political and territorial expansion. This insistence probably flows from a set of assumptions held by those students of international affairs who consider themselves "realists." To them politics and power are the primary preoccupations of governments. The seeking of economic advantages is a decidedly secondary objective.

Students who comprise the William Appleman Williams school of diplomatic history would reverse that order. Should one of them study Dowty's evidence he would probably conclude that Pierce failed to expand politically into the Caribbean, Central America, and Hawaii simply because that goal was not uppermost in his mind. The president and his State Department dedicated themselves instead to the real function of nineteenth-century American diplomacy, enlarging the sphere and profitability of overseas American economic activity.

KENNETH J. HAGAN

United States Naval Academy

ANNE L. AUSTIN. *The Woolsey Sisters of New York: A Family's Involvement in the Civil War and a New Profession (1860-1900)*. (Memoirs of the American Philosophical Society, volume 85.) Philadelphia: the Society. 1971. Pp. xv, 189. \$3.00.

Specialists in women's history and the Civil War as an institutionalizing force will find material in this chronicle of the private lives and public activities of Abby, Jane, and Georgeanna Woolsey. Since the author's purpose is somewhat narrowly defined, however, as a desire to win proper recognition for the Woolseys' contribution to the professional nursing movement and other forms of social welfare, the

book is difficult to assess because, while achieving her stated purpose, she leaves undeveloped significant possibilities inherent in the material spread out before the reader.

The Woolseys were an affluent, cultivated family of ancient lineage and distinguished connections, living in New York before, during, and after the Civil War. Seven of the eight children of Charles and Jane Woolsey were daughters, born between 1828 and 1839, and coming to adulthood in the 1850s when feminine assertiveness reflected the broadened education and rising expectations of some urban middle-class women. The author draws on an amply filled storehouse of family letters, diaries, journals, and privately printed memoirs for a sketchy portrait of the educational background, social environment, intellectual, social, and political interests of the seven sisters. Their individuated life styles reflect the measure of their liberation.

After the fall of Fort Sumter, Abby, Jane, and Georgeanna Woolsey were totally committed to war work. Abby was associated with Dr. Elizabeth Blackwell in organizing the Women's Central Association of Relief, forerunner and source of the initiative out of which came the U.S. Sanitary Commission. Jane and Georgeanna pioneered in the emergency hospital camps near Washington, finding ways to make themselves useful, as self-appointed angels of mercy amidst the hapless and unhygienic confusion during the formative months of the Sanitary Commission's hospital-related activities. Their letters and reports interpreted the needs, for Abby's use on the home front, and offered fleeting glimpses of Frederick L. Olmsted, Dorothea Dix, and assorted samples of obtuse officialdom. They documented the incredible medical and dietary procedures that made hospitals far more dangerous than battlefields and furnished a record of the development of nursing service and hospital management from Ball's Bluff to Gettysburg.

The experiences of the Woolseys, in many respects parallel to those of Florence Nightingale in Crimea, gave rise to the conception of the need for professionalized nursing education. Florence Nightingale provided the model for the schools of nursing education established by the Woolseys in New York and New Haven in 1873, with stiff criteria for entry and high standards for training, and for the subsequent development of nursing as a professionalized occupational sphere for women.

LOUISE M. YOUNG
Washington, D.C.

MILTON PLESUR. *America's Outward Thrust: Approaches to Foreign Affairs, 1865-1890*. DeKalb: Northern Illinois University Press. 1971. Pp. vii, 276. \$12.50.

Among the growing volume of recent works on the origins and history of American imperialism, we are fortunate to have the present study on the last third of the nineteenth century. When so much of the literature is nationally deprecating and (in the name of economic determinism) preoccupied with proving the influence of special interest groups upon foreign policy, it is refreshing to find a scholar seriously exploring a range of determinants in a more balanced approach.

Plesur's main theses are that the United States did not suddenly and almost unwittingly burst onto the imperial scene (a myth highly improbable on its face); that the molders of public opinion had as much to do with promoting a more aggressive world policy as did exporters and shipping lines (the evidence is substantial); and that indeed, ideology frequently overcame palpable economic self-interest in moving the nation toward overseas territorial expansion over the objection or misgivings of the commercial establishment. The technique is what the author calls an "attitudinal study," generalizations being made upon random samplings of newspapers, trade and professional journals, and other indices of public opinion. What the traditional—and the more radical—student of diplomatic history will find missing is archival documentation on the one hand, and statistical correlation on the other.

Familiar ground is covered in such topics as American territorial interests in Santo Domingo, Cuba, Hawaii, Samoa, the Congo, and the Orient. The exploration of the same theme in the Middle East and in the international conference movement is more novel. The Spanish-American War is treated as a culmination of a chain of precedents, not as "the great aberration" of foreign policy. One could argue with the allotment of space to the various topics, and the volume unfortunately projects the image of a collection of somewhat unrelated chapters. However, it is a valuable counterbalance, and it should be useful in updating such works in the field as those by David M. Plecher and Walter LaFeber. But there is yet more light to break forth upon American foreign relations in the late nineteenth century.

O. LAWRENCE BURNETTE, JR.
Stratford College

ALWYN BARR. *Reconstruction to Reform: Texas Politics, 1876-1906*. Austin: University of Texas Press. 1971. Pp. xiv, 315. \$8.50.

For many years the literary vehicle most commonly used for late nineteenth-century United States political history was either biography or monographic studies of Reconstruction, Populism, Greenbackers, and the like. In either case writers had a natural tendency to exaggerate the importance of the man or the movement about which they were writing. Recently, excellent reviews of state politics have emerged that take a somewhat broader and more comprehensive view of the period and that successfully meld the major and minor personalities and movements into an understandable whole. Alwyn Barr's *Reconstruction to Reform* is such a book about Texas politics, and the author is to be commended for a job well done.

Barr's point of departure is 1876, the year the adoption of the Bourbon constitution closed the door on Texas Reconstruction. Vestiges and overtones of Reconstruction racial politics remained for years to come, of course, but Barr is essentially correct in declaring that after 1876 economic and sectional disputes eclipsed race as the major issues dividing people and politicians. The study terminates with the adoption of the Terrell election laws, approved in piecemeal fashion shortly after the turn of the century, which noticeably affected the contest for governor for the first time in 1906.

One wishes Barr had begun his study with 1865 rather than 1876. His two short introductory chapters dealing with the election of 1876 infer an understanding and knowledge of Reconstruction that would have erased, perhaps, many of the Dunning school assumptions under which Texas history of that period still suffers. However, the post-Reconstruction period provides a stimulating challenge to historians, and Barr grapples with those less emotional, but fundamentally more important, issues of commercialization of agriculture, growth of business monopoly, and the closing of the frontier. The agrarian revolt and third-party movements that developed in response to these economic forces are reviewed in a thoroughly professional and competent fashion. We are apt to learn more about the role black people played in those years when monographic studies now underway are completed, but for the moment Barr is thoroughly conversant with the most recent writings on that point and, except for this one area, his book is apt to be the standard for the period for many years to come.

JAMES A. TINSLEY
University of Houston

CARL H. CHRISLOCK. *The Progressive Era in Minnesota, 1899-1918*. (Minnesota Historical Society, Public Affairs Center Publications.) St. Paul: the Society. 1971. Pp. xiii, 242. \$7.50.

DAVID P. THELEN. *The New Citizenship: Origins of Progressivism in Wisconsin, 1885-1900*. [Columbia:] University of Missouri Press. 1972. Pp. 340. \$12.00.

Scholars of the early twentieth-century Progressive era in the United States have often dreamed a dream. It seemed likely that once research and writing moved from the area of national politics, biography, and generalized intellectual history to state and local studies many of the glaring disagreements on fact and interpretation among established scholars in the field would be resolved. At present there are about three dozen published studies of state and municipal social and political developments during the era, together with ten times as many unpublished ones. The long awaited time for historical synthesis should be at hand.

Carl H. Chrislock's *The Progressive Era in Minnesota, 1899-1918* is unlike most state studies in that it is not a doctoral dissertation; his unpublished dissertation completed at the University of Minnesota in 1955 was "The Politics of Protest in Minnesota, 1890-1901: From Populism to Progressivism." The present book is a well-documented work by a mature scholar of not inconsiderable literary talents. As the author concedes, it belies its own title by skipping lightly over the period when Progressivism was at its height (so as not to duplicate published biographies of Progressive governors) and focuses on the circumstances and consequences of the movement's decline.

David P. Thelen's *The New Citizenship: Origins of Progressivism in Wisconsin, 1885-1900* is much more like previous state studies. A Ph.D. dissertation initially, it is literate, though hardly literary, and rests on prodigious research. The work developed from Thelen's prize-winning master's essay at the University of Wisconsin, *The Early Life of Robert M. La Follette, 1855-1884* (1966), the research for which convinced him that Wisconsin's Progressivism could not be understood in terms of La Follette's leadership, quite the contrary. But, unlike many state and local studies, Thelen's book is not just a survey of social and economic developments melded vaguely with political administrations; it is brilliantly, even belligerently interpretive. Chrislock, on the other hand, begs off the kind of interpretation that might aid synthesis. After a brief, witty summation of interpretive controversies about national Pro-

gressivism, he concludes: "My position is flexible. To a greater or lesser extent . . . [Progressivism] may have been all of these things."

Though the books deal with Progressivism in individual states before it existed or after its peak, both authors believe their conclusions to be relevant to Progressivism generally. As such the works suggest how difficult the eventual work of synthesis is going to be. Take, for example, as most previous studies have insisted, the peerless Progressive leader, Robert M. La Follette. Chrislock continues to regard him as such, but Thelen sees the Wisconsinite as an opportunist whose Progressivism was largely an "out politician's" response to the rising progressive mood. Chrislock uses La Follette, declaring that the "name was almost synonymous with radical progressivism," as a foil for disputing the view that Minnesota's governor, John A. Johnson, was "a reformer par excellence." Yet Chrislock's criticisms of Johnson in contrast to La Follette are virtually identical to Thelen's charges against the Progressivism of the Wisconsin leader.

Chrislock faults Johnson for, unlike La Follette, not dedicating "his career to . . . crusades for righteous causes," for having "consistently adjusted to his surroundings," for too much of a "capacity for establishing rapport with all kinds of people," and for having earned the "harshest criticism . . . from a group of advanced progressives." Anyone searching for synthesis would have to conclude that if Thelen is right about La Follette (I believe he is not), then Chrislock is wrong about Johnson, or vice versa.

More of an obstacle to synthesis even between two Midwestern states so similar as Wisconsin and Minnesota is the scholars' views of what large city, small town, and rural groups contributed to the movement. Thelen puts great emphasis upon the leading role of Wisconsin's largest city, Milwaukee, while at the same time arguing that "size of home town" had little, if any, relevance to the making of a reformer. Chrislock, on the other hand, declares categorically: "The main push came from the nonurban middle class. Small-town Minnesota supplied most of Progressivism's visible power structure." The contradiction may be partly resolved by an interesting proposition that Thelen generalizes for several states other than Wisconsin, a proposition that Chrislock's data suggests may have been true of Minneapolis and St. Paul. Urban Progressivism is held to have prospered more in states like Wisconsin with a single, dominant metropolis than in states where two or more urban centers competed for

hegemony. Even so, such sharp contrasts between the movements Chrislock and Thelen describe exceed by far the differences their analyses make evident between the two states.

Chrislock's failure to attempt interpretive rigor saves him from criticism for much beyond that failure; Thelen's indefatigably rigorous effort at interpretation opens him to challenge. He feels that established scholars have failed to see the importance of consumer and taxpayer protest at the local level. The early Progressives' orientation as consumers and taxpayers led them to doubt "whether the existing political economy could ever meet their needs" and led them, through an admirable resort to "direct democracy" and such devices as the income tax, to try to "produce fundamental changes." One boggles a bit, with so bold a thesis, at not finding consumers mentioned again until a third of the way through a 312-page book. They receive throughout, in fact, rather sparse attention. Thelen actually is far more preoccupied with a corollary to his main thesis, that it was the depression of 1893 that brought producer, ethnic, and what-have-you groups in Wisconsin together as beleaguered consumers and taxpayers to open the way to Progressivism. He concludes bluntly: "Without the depression of 1893-1897 there would have been no Wisconsin progressivism," certainly a strong enough Q.E.D. for a clearly stated hypothesis. But one wonders why Thelen failed to consider alternative hypotheses. It is possible to argue, as some have, that financial panics and depressions repress reform desires in the very urban, middle-income groups that Thelen emphasizes. A large part of his assiduously collected evidence can be used to support that very proposition. Had Thelen weighed his evidence in the light of such a hypothesis, as well as his own, his case might not be, as it is now, unproven. In sum, both books, for all their excellence, are disappointing as works of interpretation.

CHARLES FORCEY
State University of New York,
Binghamton

YONATHAN SHAPIRO. *Leadership of the American Zionist Organization, 1897-1930*. Urbana: University of Illinois Press. 1971. Pp. xi, 295. \$9.50.

SAUL S. FRIEDMAN. *No Haven for the Oppressed: United States Policy toward Jewish Refugees, 1938-1945*. Detroit: Wayne State University Press. 1973. Pp. 315. \$15.95.

Despite differences in tone, method, and sub-

ject matter Friedman's impassioned study of the relationship between American Jews and their government in the era of the Holocaust and Shapiro's cool assessment of American Zionist leadership are intimately bound. The virtual annihilation of Europe's Jews during World War II and the emergence of the state of Israel have crystallized now, a generation later, a new revisionist American Jewish historiography of which these two significant books are representative.

A Columbia-trained Israeli teaching historical sociology at Tel Aviv University, Yonathan Shapiro brings the sophisticated perspective of the gifted outsider-insider to his study of the American Zionist movement to 1930 when what he calls "Palestinianism"—"all Jews should help in the upbuilding of Palestine as a national Jewish home"—replaced earlier aspirations for Jewish cultural solidarity and became the official ideology of the American Zionist Organization. Shapiro asserts that "Palestinianism" became the price of American Jewish acculturation, "the cornerstone of a specific American Jewish culture" (p. 6), and the continuing basis for Jewish group integrity. A student both of Sigmund Diamond and Robert K. Merton, Shapiro gives particular attention to "the changes in the status-set and role relations of the members" (p. 4) of the ZOA and the American Jewish community that brought this about. Within the bounds of his own sociological parameters Shapiro's analysis is most persuasive. The historical sociologist, however, stumbles badly when the ventures into the realm of biography to cement his argument. For Shapiro, Louis D. Brandeis is the prototype of the marginal man turned ethnic politician, the inauthentic leader "from the periphery" who singlehandedly transformed Zionism into Palestinianism. Shapiro insists that the outstanding leader of American Zionism "around whom all Zionist activities in the United States revolved from 1914 to 1921" (p. 99) almost barefacedly co-opted the Jewish electorate to advance his own public career. Frustrated in his ambition to become attorney general of the United States in 1912, Brandeis promptly and conveniently became a political Jew and Zionist to ensure his appointment four years later to the United States Supreme Court, the first Jew to be so designated. The circumstantial evidence for Shapiro's conclusion is a classic illustration of the *post hoc, ergo propter hoc* fallacy. Alpheus Mason, Ben Halperin, Stuart Geller, and Melvin Urofsky have demonstrated that this proud scion of a well-known Jewish

family was driven to champion the powerless in movements that coincided with his own austere set of liberal-populist values. Brandeis, a son of immigrants from Bohemia, a native of Kentucky, and a student in Germany, changed his middle name from David to Dembitz in 1882 upon his appointment as lecturer to the Harvard Law School. This was a tribute to the only professedly Jewish member of his family, his uncle, Lewis Dembitz, a Lincoln elector, an honored attorney, an orthodox Jew by conversion, and an early Zionist. As "the People's Attorney" Brandeis was a remarkably authentic if unconventional Jew, his opaque style and demeanor veiling the fiery historical consciousness of a born loner. A biographical study of American Zionist leaders is sorely needed to lend depth and subtlety to Shapiro's provocative exercise in historical marginality.

For Saul Friedman, who teaches ancient and Near Eastern history at Youngstown State University, there are also no heroes. Coming after studies by Arthur Morse, David Wyman, and particularly Henry Feingold, *No Haven for the Oppressed* is a soul-searching and dramatic if fruitless effort to identify American Jewish culpability for the Holocaust. Like his "anomic" fellow Jews, Brandeis's stentorian successor and the nation's most well-known rabbi is portrayed as a quietly desperate victim of the isolationist tornado of the 1930s, of his friendship for FDR, of the deep-seated parochial divisions among American Jews, and of general indifference in an era of catastrophe to the tragic fate of his co-religionists in Europe. In September 1942, upon learning of the impending Holocaust, Stephen S. Wise privately despaired to his longtime friend, John Haynes Holmes, but publicly remained silent. "Think of what it means to hear, as I have heard, through a coded message—that Hitler plans the extermination at one time of the whole Jewish population of Europe, and prussic acid is mentioned as the medium" (p. 142).

These two books are valuable because they focus on the breakdown of a larger Jewish and of course Christian consciousness in the face of ultranationalism gone mad. Their relentless moral probing of the liberal assumptions that proved powerless to prevent the destruction of two-fifths of the world's Jews, however, leaves too much margin for innocence even as their episodic methods limit their value as fully realized history.

MOSES RISCHIN

San Francisco State University

ROBERT B. CARSON. *Main Line to Oblivion: The Disintegration of New York Railroads in the Twentieth Century*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1971. Pp. ix, 273. \$12.95.

This is a needed, informative, and thought-provoking (if not always convincing) book. It is needed because it fixes the spotlight on the railways' modern problems of excess capacity and competition. Although these are by no means the only problems (one might mention overregulation, discriminatory taxation, outmoded working rules, and the like), they are certainly at the center of the stage. Dr. Carson diligently traces the onset and growth of these twin problems in the state of New York. But he refers frequently enough to out-of-state and federal developments to place his microcosmic study in its proper setting.

This book is not only informative but, in the best sense, encyclopedic. There is nothing like it as a brief account of the big roads. Whereas it does not pretend to be all-inclusive, it provides not only facts but interpretations that force the reader to think. For example, are today's problems largely of the industry's own making as Dr. Carson says they are? After the Civil War were roads built "without much regard for the routes" or for costs? (p. 20). Was regulation invoked primarily to control competition and (contrary to what Sharfman says) largely by the roads themselves? Is it true that prior to 1920 railroads "rarely" used sinking funds, refunding programs, or depreciation allowances? (p. 58). Did the ICC subordinate itself to the policies of the railroads in the 1930s and thereafter? Has "no large American industry ever had the special access to the public treasury that the railroads . . . enjoyed?" (p. 113). Were the railroads, as late as 1945, "virtually a protected public monopoly?" (p. 130). Is it true that railways, in abandoning hopelessly money-losing lines, "never calculated the community or social costs they created?" (p. 220).

I would hardly answer these questions as does Dr. Carson; there is a great deal to be said on both sides of most of them. Yet the author has done a great service in raising the issues, for they do go to the heart of the present confused and serious situation. Despite his evident distaste for the balance-sheet policy making of the roads, he strives mightily at least to recognize both sides of any given question. This is all to his credit.

In the volume's very first sentence the author

asserts that rail history has been written "almost to the complete exclusion of twentieth-century developments" (p. 3). Not so. At least a third of the printed works now available are wholly or in part concerned with the twentieth century. Had Dr. Carson consulted some of the many key works omitted in his bibliography (e.g., Sharfman's *The American Railroad Problem*), he might well have avoided some of the oversimplified generalizations in the text. On the other hand, it is again to his credit that he has avoided counterfactual nonsense and meaningless models; neither would have been appropriate in a work of this sort. This is, rather, an inductive, "tell-it-like-it-is" account. It will, I think, evoke a few painful catcalls along with some well-deserved plaudits, but, in any event, we will all be the wiser for it.

RICHARD C. OVERTON
Burr & Burton Seminary

ARNOLD H. TAYLOR. *American Diplomacy and the Narcotics Traffic, 1900-1939: A Study in International Humanitarian Reform*. Durham, N.C.: Duke University Press. 1969. Pp. viii, 370. \$11.00.

While concentrating upon the first four decades of the twentieth century Taylor traces American interest in the international control of narcotics from the Opium War of 1839-42 to the late 1960s. Missionaries and reformers urged the American government to initiate diplomatic moves to end the opium trade with China, and the government was often responsive to their wishes. The United States took an active role in the Shanghai Opium Commission of 1908, and an American diplomatic initiative resulted in the Hague Opium Conference of 1911 and 1912, which concluded a far-reaching convention. After the First World War the United States and other Western powers came to realize that opium was not the only narcotic in need of international regulation; cocaine and Indian hemp were becoming problems. And the Western powers came to realize that the Far East was not the only part of the world in which narcotics were claiming their addicts. The United States and several European countries also had problems with drug addiction and the importation of narcotics. During the 1920s and 1930s the United States cooperated closely with the League of Nations to extend international controls.

This book is the result of intensive research in State Department documents and other records. It offers no unusual thesis or inter-

pretations, but there are provocative discussions of connections between economic problems in narcotics-producing regions and the diplomacy of narcotics control, and the accounts of American cooperation with the League make a considerable contribution to an understanding of relations between the United States and the League. This book merits the attention of students of American diplomacy, and it should find a place in the library of everyone engaged in present-day efforts to control addictive drugs.

CALVIN D. DAVIS
Duke University

ZOSA SZAJKOWSKI. *Jews, Wars, and Communism*. Volume 1, *The Attitude of American Jews to World War I, the Russian Revolutions of 1917, and Communism (1914-1945)*. New York: Ktav Publishing House. 1972. Pp. xxvii, 714. \$20.00.

What we have here is the publication, in book form, of Zosa Szajkowski's research notes. These notes cover aspects of Jewish history between 1914 and 1945. Most of his topics (but not all of them) deal with American Jewish opinion; most of them (but not all) with that opinion as it touches American-Russian relations. He examines radical opinion most thoroughly, but is by no means confined to that alone. There are thirty-four chapters, ranging in length from over 30 pages to under 3 pages; and it is exceedingly difficult to discover any cogent relationship between such topics as "European Jewish Attitudes to the War" and "Jews and the New York City Mayorality Election of 1917," between "Greenwich Village and the Lower East Side Radicals" and "Soviet Persecution of Zionism."

As with any collection of research notes, much depends on how widely the researcher has looked and how interesting and important were the things he chose to gather. In both of these matters Szajkowski performs admirably. He has examined prodigious quantities of primary sources—dozens of personal collections, the Yiddish press and sizable portions of non-Jewish newspapers and periodicals, government documents, and the papers of many Jewish and non-Jewish organizations. One cannot accuse him of any lack of diligence. Moreover, he has a wonderful eye for the dramatic, the poignant, the startling. The book is a mine of often quite remarkable material and readers will find hours of fascination in its pages.

Nevertheless, it has all the disadvantages of an undigested collection of notes. Almost half of the book (including the 150 pages of closely

printed notes) consists of long indented quotations. There is little exercise of discretion and virtually no judgment about what is important and what is not: why the chapter on Judah Magnes's views deserves 30 pages while chapters on "The Versailles Treaty" and "World War II" are each dispatched in 6 pages is never explained; why the problem of Russian re-emigration after 1917 deserves extensive treatment while there is no systematic consideration whatsoever of, say, the Balfour Declaration or the Russian Civil War is also unclear. One is left with the feeling that the author devoted a lot of space to things for which he had a lot of notes and a little space to things for which he had only a few. Thus, although the subtitle indicates that the study will cover three decades, over three-quarters of the text deals with the first five-year period.

Szajkowski's own thesis statement is unsatisfying. "My sole purpose," he writes, "is to prove that there was never a monolithic attitude of American Jews to radicalism" (pp. xx-xxi). While this formulation seems to legitimize the indiscriminate introduction of random contemporary opinions, Szajkowski is obviously doing battle against a proposition—that all Jews were radicals—which few scholars can seriously espouse. It is hard to imagine even a superficial student of the problem who could believe that Jacob Schiff and Emma Goldman, that Morris Hillquit and Bernard Baruch, that Sam Gompers and Lillian Wald ever had monolithic opinions about anything.

In short, while this book is the product of extensive research and is filled with deeply interesting material, it is seriously (if not fatally) marred by its lack of proportion, discretion, judgment or unity of purpose.

DAVID W. LEVY
University of Oklahoma

JOSEPH R. STAROBIN. *American Communism in Crisis, 1943-1957*. (Prepared under the auspices of the Research Institute on Communist Affairs, Columbia University.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xvii, 331. \$12.95.

Although he participated in the events that he analyzes, Joseph Starobin's book is not an autobiography. Scholarly and well documented it rarely mentions his own role as foreign editor of the *Daily Worker* and one of the party's experts on youth. Yet it is full of the kind of insight that only direct involvement in the movement can provide.

In Marxist terms Starobin's analysis of the

party's last years is essentially revisionist. He is highly critical of William Z. Foster and the left-wing militant line adopted by the party after the overthrow of Earl Browder in 1945. ("Foster's doomsday drive," he calls it.) The party was on the verge of a breakthrough in 1945, says Starobin, with the potential of becoming an important force in American politics, but only if it followed Browder's path and turned itself into a form of "left-wing lobby." Browder, for all his mistakes, was trying to do what was necessary for communism in America: to reshape it to meet the American situation, to cut it adrift from its propensity to import its analyses, ideals, and programs from abroad.

Whereas others see the party as slavishly following the orders of Moscow and the CPSU, Starobin paints a less conspiratorial, though perhaps more depressing picture: the American Communists were "neither allies nor agents" of the Soviet Union. Indeed, for the most part, the Russians neither knew nor cared much about what they did. Direct contact with Moscow was minimal. But the party's leaders, the generation that rose in the 1920s, imprisoned it in a kind of "mental Comintern," following what they thought were the wishes of Moscow, unable to recognize that, in practice, polycentrism was emerging in the Communist world even while Stalin lived.

This was exemplified best, perhaps, with the controversial Duclos article. Here Starobin tends to reinforce his colleague at York University in Toronto, Gabriel Kolko, and the New Left interpretation of the cold war. Clearly, says Starobin, the article reflected the thinking of someone in power in Moscow, but it was based on a grave misreading of Browder's line. The article, in turn, was grossly misinterpreted by the CPUSA. Its leaders concluded, wrongly, that it called for a toughening of the line and the beginning of a cold war. The CPUSA, therefore, was virtually alone in shifting toward a more militant line and soon found itself out of step with the other Communist parties, including Duclos's.

Ultimately, says Starobin, Foster's militant line led to where it was doomed to lead: to a form of suicide, culminating in the self-destructive fiasco of the Wallace campaign of 1948.

Starobin is very persuasive, but there are aspects of his book that are open to dispute. For instance, his tendency to heap most of the blame on Foster & Co. leads him to underplay the very strong forces outside the CPUSA working toward the destruction of communism, socialism, Browderism, or any left-wing move-

ment at that time. But this is a measure of how provocative and full of ideas it is. It is also refreshing to come across a book on this subject that manages to overcome its sources, shunning what Starobin calls "the peculiar Esperanto of the Comintern," indeed translating it with grace, style, clarity, and a great deal of insight.

HARVEY LEVENSTEIN
McMaster University

ERNEST R. MAY. *"Lessons" of the Past: The Use and Misuse of History in American Foreign Policy*. New York: Oxford University Press. 1973. Pp. xiv, 220. \$6.95.

THOMAS M. FRANCK and EDWARD WEISBAND. *World Politics: Verbal Strategy among the Superpowers*. (New York University, Center for International Studies. Studies in Peaceful Change.) New York: Oxford University Press. 1971. Pp. xiii, 176. \$5.95.

Two important trends in recent analyses of postwar American foreign policy—a deep and even personal concern about the direction it is heading, and the use of insights derived from social psychology—inform both books reviewed here, albeit differently. Ernest R. May's *"Lessons" of the Past* focuses upon the role of history and historians in the formulation of this policy. Since policy makers inevitably use analogies from the past in formulating or justifying their own actions but often misperceive these past events or ignore those most relevant to their immediate needs, he argues, professional historians, by their training, knowledge, and insights, are uniquely situated to contribute substantially.

May shows how a preoccupation with events in the recent past, and beliefs about how apparently similar crises in this recent past should have been handled, shaped the policies made by American leaders: wartime planning for the aftermath of World War II, changing attitudes in the late 1940s toward the Soviet Union, the Korean intervention, and the Vietnamese War. He does not deny that other factors played a role, including traditionally anti-Soviet views of State Department experts, domestic political pressures, and even Soviet behavior. Nor does he argue that outcomes would have been substantially better or even different from those that did result had top-level officials sought more relevant analogies in history. The point is rather that selective perception of the past and its relationship to present policy needs prevented policy makers from making the best

use of historical knowledge. May then shows how its better use might have improved the quality of decisions made about bombing North Vietnam.

May is on less sure footing, however, in searching for concrete ways in which professional historians can contribute to more effective policy making. They can deliver better, more pointedly relevant information. Along with experts in other disciplines, historians, because their professional training requires them to search for critical variables accounting for change and stability in the past, can make forecasts about future developments. Meanwhile, the government can assist in making historians more helpful by providing funds to expand specialized training, arranging for tours of duty by historians in policy agencies, and, above all, adopting a more open information policy.

More problematic are psychological and structural barriers in the policy-making process. It is not clear how the availability of even the most reliable knowledge and competent advice could overcome a policy maker's tendency to rely upon what is in his personal experience; to perceive the past, the present, and policy alternatives in highly selective terms; to search out those historians as policy advisers who are most likely to produce results congenial with his own preconceptions; to create high-level policy conferences in which mutually shared predispositions are reinforced rather than questioned; to resist potential damage to personnel or policies that might result from historians rummaging prematurely among hitherto closed files. Such constraints sharply limit the contribution historians can make in planning policy.

An underlying thesis of *World Politics*, by Thomas M. Franck and Edward Weisband, is that, quoting President Nixon, "the rhetoric in international affairs does make a difference." Expanding upon a theme frequently heard after August 1968, they argue that the rhetoric and behavior of the United States vis-à-vis Guatemala in 1954, Cuba in 1962, and especially the Dominican Republic in 1965 provided a precedent that the Soviet Union could and did adapt to its own purposes to end the "Czechoslovak spring."

The volume, although marred by excessive righteous indignation, cute inaccuracies (the world as a "two-ghetto system"), and redundancy, underlines an important point: the Johnson and Brezhnev doctrines used to rationalize these interventions merely verbalized a reciprocity that had long existed. Except in the

Far East, both superpowers had delineated tacitly and openly the areas in which they would brook no interference from the other. Similarly, both refrained from direct interference in the other's delimited area.

This stabilizing reciprocity nonetheless posed dangers. For one thing, the incongruence between behavior and rhetoric—both sides acted to maintain their tacit “bargain,” but each decried as immoral and contrary to international law the actions of the other—ran the risk of inciting to rash action some foreign statesmen who took both at face value, or leading American and Soviet policy makers to forget the tacit agreement underlying the incongruence. More seriously, the stability toward which the superpowers pushed was stultifying for their satellites. The authors thus see the Nixon doctrine and initiatives as a step in the right direction toward loosening existing bipolarity. An even broader strategy, akin to Charles E. Osgood's GRIT (Graduated Reciprocal Initiatives in Tension-reduction), can synchronize behavior and rhetoric to ease tension, to build a “normative, reciprocally applied system of superpower interaction and world order.”

RICHARD L. MERRITT
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HADLEY ARKES. *Bureaucracy, the Marshall Plan, and the National Interest*. Princeton: Princeton University Press. 1972. Pp. xiv, 395. \$12.50.

The author's stated aim in this volume is to use the Marshall Plan as a case study of the way in which the national interest comes to be defined in the design of a major foreign policy program. His approach is to focus on the way in which problems of administration were handled initially when the program was being designed in the executive branch and in Congress and finally by the way that the Economic Cooperation Administration handled them in administering the Marshall Plan. The principal thesis of roughly the first half of the book, which deals with the initiation and enactment of the program, is that the way in which substantive policy questions were encountered and resolved was in the debate of issues that first presented themselves as administrative in character. The elaboration of this thesis culminates in a list of policy “themes” and a collection of “operating rules” in the form of presumptions intended to guide the ECA in carrying out the plan, all of which are demonstrated to have been explicit or implicit in the legislation, or in

other documentation, and to have been evolved, for the most part, in the consideration of administrative matters. The thesis is well sustained and its development provides, at least to me, original insights into the process of policy definition.

In the balance of the volume the author addresses himself to the question of whether the policies, the operating rules, and the priorities among them, the development of which he has traced, were actually applied in practice by the ECA. Broadly speaking (if I read him correctly) his answer comes out in the affirmative. Although there is little to question in the account of the ECA's operations this part of the book is less satisfactory, at least to one who was a participant in the program. Perhaps the difficulty is that because the discussion is organized around issues of administrative procedure and the analysis of the efforts made to achieve substantive economic objectives is fragmented, matters of secondary interest receive too much emphasis and the treatment of those that actually occupied the energies of the agency is inadequate.

For instance, the author is perceptive in explaining that the ECA was neither able nor eager to influence the behavior of European governments or private businesses at the level of individual projects or to control the commodity composition of their imports. That the recipient countries had sufficient foreign exchange resources of their own and sufficient domestic capital other than counterpart funds to preclude U.S. intervention at this level was, however, clearly understood as a fact of life from the beginning. It did not seem to be particularly significant as a deliberately adopted policy.

On the other hand, in the discussion of U.S. pressure for economic integration in Europe, for restraints on consumption required to permit a high rate of investment, and for increased productivity, the author touches on objectives that were of vital interest to the agency. Greatly underemphasized, however, was an objective related to, but in some ways more pressing than any of these; namely, the suppression of inflation. The achievement of these objectives was understood by all concerned to depend upon the adoption of appropriate monetary, fiscal, and trade policies. Appropriately, therefore, the concerns of both Americans and Europeans were with these broader areas of economic policy. The author's treatment may understate the degree to which U.S. influence was brought to bear in them.

An author is entitled to define his own subject matter. This author chooses in the second part of his volume to review the application of virtually all the policies and operating rules, the origin of which he discusses in the first half. As it seemed at the time, however, and still seems in retrospect, many of these had little relevance to the task at hand, except for their nuisance value, and the attention devoted to them in this book seems to be at the expense of a more coherent analysis of the way in which and degree to which the really important policies were implemented.

RICHARD M. BISSELL, JR.
Farmington, Connecticut

DAVID EDWIN HARRELL JR. *White Sects and Black Men in the Recent South*. Foreword by EDWIN S. GAUSTAD. Nashville: Vanderbilt University Press. 1971. Pp. xix, 161. \$6.50.

This book is a study of the evolution since World War II of the racial views of those Protestant sects whose greatest appeal is to whites from the lower socioeconomic strata in the South. It is an insider's view in that the historian includes himself among educated young sectarian leaders who have "self-consciously searched their past to try to explain their church's record of racial prejudice and injustice and try to find precedent for liberal social action" (p. 118). Emphasizing sociology rather than theology as the basic determinant of the racial views of the sects, he explains the trend toward moderation in terms of the social ascent and striving toward respectability of significant numbers of members of the larger sects. At the other pole he explains the trend toward racial integration of the smallest, weakest sects in terms of lack of resources to maintain segregated facilities and institutions.

While interesting data is assembled to support these conclusions, the book suffers from overreliance upon publications of the various sects and public pronouncements by their leaders. More varied sources, particularly oral history interviews with both leaders and members, could reach a deeper level of truth. Moreover, the internal evolution within the sects is treated as if it were only slightly related to happenings outside: a defect that seriously weakens the analysis and undermines the long-range value of this work.

GWENDOLYN MIDLO HALL
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Rutgers University

HERBERT S. PARMET. *Eisenhower and the American Crusades*. New York: Macmillan Company. 1972. Pp. xi, 660. \$12.95.

In the final sentences of this perceptive book the author writes the best short summary of his subject: "To label him a great or good or even a weak President misses the point. He was merely necessary" (p. 578). The preceding pages of the detailed narrative prove his point.

The opening of many personal and official papers of the Eisenhower presidency permits Professor Parmet to go well beyond earlier accounts based on contemporary newspapers and periodicals and the earlier memoirs by participants in the events of the period. The author has drawn heavily on his interviews conducted in the 1969-71 period with twenty-five associates of the general's and on larger collections of interviews at Columbia and Princeton relating to Eisenhower and to Secretary of State John Foster Dulles.

The legend that "Ike, the simple soldier," merely supplied a smiling façade for administrations dominated by leaders such as Sherman Adams and Secretary Dulles is effectively denied. Long before Dulles's illness made him ineffective in foreign affairs the president made his influence felt in the formation of American policy. A world leader who had talked with presidents, prime ministers, and kings and played a major role in international affairs at SHAEF and SHAPE did not have to defer in such matters to his secretary of state.

The general's political astuteness (obviously marred in Parmet's eyes by occasional slowness in taking a firm stand) is seen in his dealings with Senator Joseph McCarthy, a darling of the right wing of Eisenhower's party. Although at times, as in the case of General Marshall, the president saddened his friends by delaying his rebuke to the Wisconsin senator, he ended by rallying the forces that destroyed the power of that politician.

From the moment he committed himself to the preconvention fight in 1952 until the end of his presidency, Eisenhower faced opposition to part of his program by conservative leaders of his party. The general's sweeping personal victories were interpreted as public mandates for a conservative program and the failure of Republicans to control Congress after the 1956 landslide was blamed on Eisenhower's acceptance of some Democratic measures. In both terms he had to turn to Democratic leaders such as Lyndon Johnson and Sam Rayburn to

salvage some of his principal proposals from attacks by members of his own party.

With an eye on recent politics the author underlines the admixture of coolness and correctness that marked Eisenhower's enthusiasm for his vice president. But he gives Mr. Nixon high marks for his effective efforts in reducing some of the right-wing onslaughts on parts of the president's program.

The Republicans, Parmet suggests, had been too long out of power. The habit of opposing the White House threatened to be carried over into a Republican administration. A mixture of accommodation and firmness—administered by an astute leader—was called for. Franklin Roosevelt had won praise for his ability to push forward liberal measures without totally alienating some of the conservatives of his party. Parmet reminds us that Eisenhower in his own way could also combine the qualities of the Lion and the Fox.

FORREST C. POGUE
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JONATHAN TRUMBULL HOWE. *Multicrisis: Sea Power and Global Politics in the Missile Age*. Cambridge, Mass.: MIT Press. 1971. Pp. xii, 412. \$12.50.

In his study *Multicrisis*, Commander Howe deals with the ability of the United States Navy during the nuclear age to provide meaningful deterrence in one area of the world while it is actively involved in another. His discussion focuses on the Middle East crisis of 1967, when the Sixth Fleet was positioned to deter Soviet intervention against Israel while the navy was also fighting in Vietnam, and on the Quemoy crisis of 1958, in which the Seventh Fleet demonstrated to discourage the Chinese Communists from occupying Quemoy shortly after the Americans had landed in Lebanon. Although American naval deterrence was by no means as overwhelming in 1967 as it had been in 1958, Howe concludes that it was effective in both instances and that in neither case were the navy's capabilities seriously compromised by its commitments elsewhere. In 1967, as in 1958, the British naval presence outside Europe was an important stabilizing factor, according to Howe. Soviet naval power markedly increased during the decade between the two crises without, however, challenging the ultimate capacity of the United States Navy to provide effective deterrence. In both crises the United States was associated with partners in the areas, Israel and Nationalist China, who demonstrated conspicu-

ous ability to care for themselves and who made commitments that the United States was unable or unwilling to prevent.

Howe warns that the navy's deterrent capacity in the third world will be increasingly limited hereafter by expanding Soviet naval power, by the growing obsolescence of American warships, and by the British withdrawal to European waters. Nevertheless, he concludes that the United States must maintain "viable . . . strength throughout the world" to sustain "existing commitments" and to prevent "policy alternatives in future multicrisis situations" from being "significantly restricted" (p. 346).

Howe's study is really more a series of estimates and statements of belief than a book of history. For American naval thinking, the commander draws from revealing interviews with persons who were close to or actual participants in the events themselves as well as on his own perceptions as a naval officer. He is unable, however, to cite the basic records essential to a full and accurate understanding of the American naval position, and his estimates of the responses by the friends and foes of American deterrence are necessarily grounded on extremely flimsy evidence. Often tediously redundant his text is burdened with the language of the experts and endowed with numerous charts of capabilities, conveying an impression of scientific precision surely far beyond what Howe intends. None will deny, however, that Howe's book is a completely genuine expression of American naval opinion that may prove very helpful to historians once they are able really to study the role of the navy in the third world.

WILLIAM R. BRAISTED
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HARRY EDWARD GRAHAM. *The Paper Rebellion: Development and Upheaval in Pulp and Paper Unionism*. Iowa City: University of Iowa Press. 1970. Pp. xv, 170. \$6.50.

The rebellion that is the focus of this book involved West Coast locals of the United Papermakers and Paperworkers and the Pulp, Sulphite and Paper Mill Workers. In all, just over 20,000 workers were involved in this 1964 separation from the established international unions in the paper industry. It does not make a very exciting tale, and the essentials could have easily been stated in an article. Only part of the book is devoted to a rather uninspired summary of the early history of the unions in

the industry. Graham's work is, however, not really a history but rather an analysis of what he perceives as a test of trade-union structure.

This kind of schism represents a rather unfamiliar form of dissent in American trade unions as compared to so-called rank and file movements or dual unionism. In this case the author convincingly demonstrates that schism was a result of a matured union bureaucracy that proved too inflexible to yield to demands for greater local autonomy and internal democratization. Furthermore, the central union leaders had enjoyed a cozy relationship with the West Coast paper companies since the middle of the 1930s and were not responsive to local demands in the 1960s for a more militant posture.

A kind of progressivism within the trade union is the solution Graham calls for, with local autonomy, recall elections, and even a form of the referendum. While such changes are undoubtedly a step forward the author does not question whether reforms that proved relatively ineffective in democratizing political institutions can be any more effective within the trade unions.

Research for this book was minimally sufficient, but certainly interviews might have added further dimensions to the study. One closes this book with the sound of scissors and the smell of paste most evident.

STEPHEN J. SCHEINBERG

Sir George Williams University

CANADA

IRVING M. ABELLA *et al.* *The Influence of the United States on Canadian Development: Eleven Case Studies*. Edited by RICHARD A. PRESTON. (The Committee on Commonwealth Studies of Duke University, publication number 40.) Durham, N.C.: Duke University Press for the Duke University Commonwealth-Studies Center. 1972. Pp. xii, 269. \$7.75.

This book is one of the first fruits of the Association for Canadian Studies in the United States, a collection of papers presented at the association's inaugural conference in 1971. The editor, Richard A. Preston, points out in his introduction that "development" is used in the title in its general and traditional sense to mean "the process of overall change that has produced the community known as Canada." The selection of papers demonstrates the breadth of this definition. The subjects range from Preston's and Carl Berger's stimulating essays on aspects of the intellectual history of

the Canadian-American relationship to studies of American influences on Canadian politics, the recent growth of sociology and political science in Canada, the Auto Pact, Maritime underdevelopment, the Canadian trade-union movement, and to such narrowly technical topics as American participation in early irrigation schemes in central British Columbia and the degeneration of the French language in Windsor, Ontario.

This diversity of theme and approach is the book's weakness, but it is a forgivable one. The object of the conference and the collection was to promote studies of Canada in the United States that move beyond Canadian-American relations into the more subtle realms of domestic Canadian life. The theme of American influences on Canadian development is a neglected and fruitful one for examination, and so pervasive that almost every corner of Canadian history invites study from that perspective. The temptation to display the widest range of examples was understandable, but it results in a loss of force in the collection. Having made this demonstration the association might usefully organize its succeeding conferences around more tightly defined themes.

Despite its lack of focus the book does assist in opening up a number of paths of central inquiry. Richard Preston argues for intensive study of "the assimilating effect of American influence on Canadian life" as one important means of judging the prospects for an independent Canadian existence; Carl Berger notes the effects of cultural relativity on the Carnegie studies of the late 1930s and 1940s in Canadian-American relations (and by implication warns of similar pitfalls in scholarship undertaken in the very different atmosphere of the 1970s). What scholar—at least, what Canadian scholar—would be inspired today by the faith of J. T. Shotwell? "The role of history and the social sciences was to reveal those processes of civilization which made all peoples one, and to destroy the outdated conceptions of national sovereignty which were the causes of war. For Shotwell, the history of Canadian and American relations became a testament to the internationalist faith—that economic interdependence and the unrestricted flow of the forces of liberal capitalism, the interchange of populations, the embedding of parts of one nation within another, and the growth of a rational and nonideological approach to problems, were the sources of peace. This was the great theme which was revealed in the history of Canadian

and American relations and which conveyed a lesson to the whole world."

Robert Babcock's and Irving Abella's papers on the Canadian trade-union movement continue the valuable work of debunking certain Canadian assumptions about the sources of international unionism in Canada. If there is a common theme to be divined from the papers it is most explicit in these two: the suggestion that Canadian deference to American values, institutions, and leadership has its source as much in Canadian choice as in American assertiveness. That hypothesis requires critical study from both the Canadian and American perspectives. Its elaboration could be one of the most useful products of the dialectic introduced into Canadian studies by the current growth of the field in the United States.

DENIS SMITH
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LATIN AMERICA

BURR CARTWRIGHT BRUNDAGE. *A Rain of Darts: The Mexica Aztecs*. (The Texas Pan American Series.) Austin: University of Texas Press. 1972. Pp. xvii, 354. \$10.00.

The thorough but often insufficiently critical examination of sources—particularly sixteenth- and seventeenth-century Spanish chronicles—attributed to Burr Brundage by readers of his *Empire of the Inca* (1963) and *Lords of Cuzco* (1967) is evident in *A Rain of Darts*. Brundage describes his new work as a "political history" of the Mexica Aztecs, a story in which the principle themes are "the keen realization by the Mexica of the illegitimacy of their claim to the land" and "their expectation of the proprietary return of the god." Dust-jacket commentary acclaims the work as serious, scholarly, and the first one-volume narrative history of the Mexica.

A Rain of Darts is a fascinating, amazingly synthetic chronicle of the legendary experiences, political and other, of the peoples of greater Anahuac. Had it been published in the days of Prescott or of Bancroft it might well have passed muster as political history; in the days of Torquemada and Mendieta it would have overshadowed all. It was written with serious and scholarly intent and is our first relatively complete, one-volume narrative treatment of the Mexica from the years of their association with the Toltecs of Tula to that humiliating summer day on which they submitted to conquest by Fernando Cortés and his fellows.

For Brundage the story of the Mexica, "whatever else it may be, is a tale of midnight murders, intrigues and wild revenges." It has "a lurid quality not often met in the chronicles of nations." As he renders it, one might well agree. His Mexica were a people "adrift in a great tempest of their own making," the "eminently quiet slaves" of a psychophilosophical orientation that was provided, at least in part, by economic and geopolitical circumstance, and the vassals of their *teuctli*, warrior lords, barons, knights, and sometimes priests whose "voracious hunger for battle, boastfulness, monstrous dignity and unending search for honors" formed the dynamic in their history.

For the general reader who is willing to absorb a welter of generally unfamiliar names, places, events, and concepts, *A Rain of Darts* will be an exciting and fulfilling tale. The specialist will admire the command of chronicle literature it evidences and may be stimulated by the vivid interpretive treatment provided individuals (such as the well-known Moctezuma I and Moctezuma II and the lesser-known Tlacaelel and Moquiux), institutions, and concepts. The specialist will be distressed, however, as he attempts to grasp the intended definition of the nature and structure of the so-called Aztec state. He will find that which is proposed rather simplistic and too often defined in terms applied to medieval European models, that is, the *teuctli* as a baron, and so forth. Finally, he will find that the sources cited often do not provide sufficiently for the conclusions reached; for example, Moctezuma II had an "abnormal fear of death," "knew his vassals hated him," and "believed the *Macehualtin*" were "vicious and lazy."

In sum, read *A Rain of Darts* and advise your students to do likewise, but read it as a fascinating tale, a stimulating adventure and departure point for further study and not a definitive history.

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SHIRLEY BRICE HEATH. *Telling Tongues: Language Policy in Mexico, Colony to Nation*. (Publications of the Center for Education in Latin America, Institute of International Studies, Teachers College, Columbia University.) New York: Teachers College Press, Teachers College, Columbia University. 1972. Pp. xx, 300. \$10.00.

This is a rather extensive case study of a dia-

chronic character that depicts in well-documented terms the "official" language policy of New Spain and the Mexico that succeeds it. The shift in this policy vis-à-vis the actual developments in language learning and perpetuation reveal the importance of the language factor in the complex sociopolitical structure of a nation.

In the course of her description of policy from the sixteenth-century Franciscan missionaries to the prevailing precepts of the Revolution, Professor Heath "takes sides" in the sense of supporting in retrospect any indicated effort of the Indians to perpetuate their own language or to create of Nahuatl a *lingua franca*. She would seem to deprecate any move toward uniformity as a reactionary gesture of the elite. One may have the feeling that in supporting this position the author is at times somewhat subjective. The role of official bodies or academies is sometimes exaggerated, in my opinion, and Heath's analysis of the factors of acculturation or her acceptance of the statements of others regarding Hispanic acculturation seem to indicate at times a sympathy with normative precepts and prescriptive procedures that is not too common among anthropologists or linguists.

Thus it is that her well-researched document on attitudes toward language development and on bilingual situations throughout the history of Mexico is summarized in chapter 9, "Overview and Conclusions," with a treatise that in spots seems an advocacy of policy. As one who has spent much time in Oaxaca, I was struck by the quoted assertion that speakers of Oaxaca will use fifty Nahuatl words for every fifty Spanish, or that the Spanish of speakers in Mexico City exhibits very little influence from Nahuatl (p. 195). In the case of both cities the domestic lexicon has many Nahuatl terms, but they constitute a small fraction of the total vocabulary, and the unrelated Indian languages heard in the market at Oaxaca are Zapotec and Mixtec, neither of the Nahuatl family.

The author does not seem to separate in her consideration, as one might expect, matters of structure from lexicon or rules of spelling. Evidence now shows that the structure of American Spanish is part of a continuum of Andalusian Castilian that has changed very little in four hundred years, and the differences that are found now depend very little on substratum indigenous forces but rather on the factor of accessibility to Andalusian influences in the colonial period.

Heath's account of the important role of social scientists, many of them Americans, in

the shaping of language policy as a part of the sociopolitical outlook of the Revolution and of the reactions to this after Cárdenas is well written, but perhaps we shall find that structurally, at least, languages will continue to develop under the pressure of extralinguistic factors and that social coercion will always have a greater part to play than planning.

D. LINCOLN CANFIELD
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CARMEN VENEGAS RAMÍREZ. *Régimen hospitalario para indios en la Nueva España*. México, D. F.: Instituto Nacional de Antropología e Historia, Departamento de Investigaciones Históricas. 1973. Pp. 223.

The purpose of this study is to explain the organization, development, and role of the various hospitals for Indians started by the Spaniards in New Spain. It is surprising that some 140 such institutions are listed in the appendix, and the author does not claim that the list is complete. Little is known today about the majority of these hospitals; it is likely that some of them were temporary institutions founded during an epidemic or famine, or that they served more as shelters for the aged or homeless than as treatment centers for the sick. Substantial information has been assembled, however, on several of the larger and more important Indian hospitals. These would include the famed Hospital Real de San José de los Naturales of Mexico City (founded about 1531), the several hospitals of Santa Fe started by Vasco de Quiroga in the sixteenth century, and the Indian hospitals of the province of Michoacán. Carmen Venegas Ramírez makes it clear that as a general rule the Indian hospitals served not only as charitable and medical centers, but as vital auxiliaries in the tasks of converting Indians to Christianity and of congregating them in urban settlements where they could better be put to work and taxed.

This interesting study is a useful addition to the literature on the medical, social, and religious history of colonial Mexico. The text is brief (134 pages), but the book is well organized, carefully documented, and based in part on original manuscripts. The appendixes are lengthy (80 pages) and valuable, and they include new information on payment of the *medio real* as well as the composite list of 140 Indian hospitals. Twenty-four black and white plates offer a useful visual dimension to the student. The most thorough work on the hospitals of

New Spain is still Josefina Muriel's two-volume work *Hospitales de la Nueva España* (1956, 1960), but persons interested primarily in Indians and their hospitals will generally be pleased with *Régimen hospitalario para indios en la Nueva España*. It falls a little short, however, of being a definitive study. One would expect that additional valuable information on the subject could have been gleaned from the *Ramo de epidemias* and *Bienes nacionales* of the Archivo General de la Nación, neither of which are cited. Nor does one find included in the bibliography any of the writings of José Joaquín Izquierdo, the dean of Mexican medical historians, or of such noted scholars as Woodrow Borah, Sherburne Cook, or Alfred Crosby, Jr. All major Latin American libraries will want to acquire this book, as will any person interested in Mexican medical history.

DONALD B. COOPER
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HORST PIETSCHMANN. *Die Einführung des Intendantensystems in Neu-Spanien im Rahmen der allgemeinen Verwaltungsreform der spanischen Monarchie im 18. Jahrhundert*. (Latein-amerikanische Forschungen, number 5.) Cologne: Böhlau Verlag, 1972. Pp. vi, 328. DM 62.

One of the more important Bourbon reforms in Spanish America was the institution of intendancies, which both centralized many administrative functions and inserted a middle layer of supervision between district and central colonial authorities. Recent studies of the reform have concentrated upon individual colonies, the Río de la Plata and Peru. Now Horst Pietzschmann adds a study of the new system in the viceroyalty of New Spain. He is interested in the antecedents of the reform, especially whether the idea should be ascribed to French influence or lay more deeply rooted in Spanish precedent and need. He is further interested in implementation and the change it brought. His original study, a doctoral thesis, was fuller than this book since a substantial section dealing with other changes in administration in Mexico resulting from the general inspection of José de Gálvez has been published separately in the *Jahrbuch für Geschichte von Staat, Wirtschaft und Gesellschaft Lateinamerikas* (8 [1971]: 126-220).

Pietzschmann's study is careful and methodical, based upon detailed reading and extensive research. His conclusions may be summarized rather simply: intendants in Spain and Spanish America represented a native development

rather than implantation of the French model; the attributes of intendants under the two crowns differed markedly. The intention of the ordinance of 1786 in Mexico was to relieve the viceroy of much administration by erecting a fiscal system only nominally dependent on him and by conceding to the intendants a large measure of autonomy. In practice the viceroys succeeded in absorbing the fiscal administration into their attributes and converted the intendants into completely dependent officials intermediate between the districts and Mexico City. Centralization and gathering of information for the government were substantially advanced, but the intention of tidying up territorial divisions did not work out completely since many fiscal and ecclesiastical jurisdictions still did not coincide with the boundaries of the intendancies. The intendants were most effective in their capitals and the adjacent districts where they exercised the functions of district administrator as well. Pietschmann judges that the reform and the officials it brought in had only a small role in the remarkable economic flowering of Mexico during the last years of the colonial period. Yet the evidence for judgment is still scant. The insertion of an intermediate level of administration was an important development, one that has been continued in the present federal system.

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WINFIELD J. BURGGRAFF. *The Venezuelan Armed Forces in Politics, 1935-1959*. Columbia: University of Missouri Press, 1972. Pp. 241. \$10.00.

This book is a study of the Venezuelan military's involvement in politics during the present century, focusing primarily on the period from the death of Juan Vicente Gómez in 1935 to the presidency of Romulo Betancourt in 1959. It is offered not only as a contribution to Venezuelan history, of value in itself, but also as "an additional case study for the comparative analysis of military politics in this hemisphere." It may therefore be viewed from divergent perspectives, with divergent results.

Judged as a work of straightforward historical narrative and analysis, it merits generally high marks and should please most readers. The period covered in detail was one of painful political transitions, coups, and crises in which the military was closely involved. The armed forces participated in coups in 1945 and 1948, nulli-

fied an election in 1952, imposed outright military rule during the ten-year period from 1948 to 1958, overthrew its own regime in the latter year, and finally acquiesced (by a narrow margin) in a return to civilian administration in 1959. Burggraaff's account of these developments and of the reasons for the military's involvement in them is generally well developed, carefully researched, and clearly and thoughtfully written. Though minimal attention is paid to the effects of economic and social developments on men and events, this shortcoming weighs less than the book's positive merits.

Judged on the other hand as a contribution to the comparative analysis of military politics the book is less satisfactory. The problem derives in part from its narrative focus, in part from methodological shortcomings, in part from Burggraaff's failure to utilize any overall theoretical or conceptual analysis through which his work can be tied to existing theory on the Latin American military or to the question of military involvement in politics generally. For example, though he is apparently familiar with the literature in the field, Burggraaff does not test any of the existing generalizations on his subject or develop any "testable" generalizations of his own. Nor does he use quantification

techniques or seek to study "the military's overall role in society, their social origins, their technical capacity, or their internal institutional squabbles over positions and promotions." Thus his book is of limited utility to scholars seeking to interpret the military's political actions in terms of socioeconomic influences or through the institutional make-up of the armed forces. Finally, I refer again to the fact that the author deals in only a limited way with economic and social developments, offering little data on the country's underlying socioeconomic structure.

It is obvious that no one book can do everything, and Burggraaff should not be criticized for not answering all questions or exploring all lines of inquiry. These comments are made simply because he stresses the value of his book for purposes of general analysis and also because it is desirable that the potential reader have a clear idea of what is dealt with in his book and what is not. In sum, the book is a worthwhile contribution to Venezuelan political history, but it is of only moderate value for the comparative study of military politics in Latin America as a whole.

GLENN E. MILLER

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication, whether concerning articles, review articles, or reviews, be no longer than 300 words. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

Arnold Thackray's article growing out of his continuing study of the membership of the Manchester Literary and Philosophical Society (*AHR*, 79 [1974]: 672-709) provides a good example of overgeneralization based on the legitimate, but in some ways limited, technique of prosopography. Thackray develops a curious argument: because science was the most important field of inquiry of the Society, it was the most important field of inquiry to most of the members; and because some members of the Manchester elite belonged to the Society, "by the early nineteenth century science was established as the cultural mode of the Manchester elite" (p. 682) and therefore "science became the predominant mode of cultural expression in Manchester" (p. 681). Although the logical sequence of the argument, which I think I have fairly presented, deserves not a moment's notice, I would like to discuss the validity of some of the individual statements.

Thackray simply does not present a good case to prove that most Society members regarded science as their "cultural mode." On

the contrary, he admits that the early scientific thrust came from a few members of the medical professions and later from a handful of outstanding scientists of whom Dalton remains best known. The Society's merchants, manufacturers, and bankers seem rather conspicuous for their indifference to science, except on occasion as a "polite, indeed ornamental" (p. 705) preoccupation. It is not enough merely to note that a man joined the Society or remained a member over an extended period to prove that science was his cultural mode because other things might have motivated such behavior. An interest in literature certainly motivated some of the men under discussion. The Society initially grew out of informal weekly meetings devoted to the discussion of literary and philosophical topics, concerns indicated in the Society's title. When the Society introduced separate sections in 1836-37, one was specifically reserved for literature, the other two being for chemistry and natural history. Besides promoting learning—mainly scientific but also literary—the Society served another extremely important function, that of conferring social prestige. Thus it must be proven in each case that an individual truly joined and remained a member in order to adopt science as his cultural mode and not in order to pursue literary studies or simply to gain prestige. Thackray does not provide this proof and, given the paucity of evidence on nearly all members except the leviathans of wealth, probably cannot furnish such proof.

Nor does Thackray fare better when he begins talking about the Manchester elite. He really fails to define this elite and all but ignores the older part of it, the magistrates, manorial and parochial officers, and Anglican clergy who remained numerous, wealthy, and powerful, though not unchallenged, down to the 1830s. Even assuming that Thackray is confining himself to one segment of the urban

elite, he should have made it clear that merchants, manufacturers, bankers, and medical men in the Manchester district numbered at least in the hundreds, and more likely in the thousands, at any given time during the period from 1781 to 1852. If Thackray believes that all of the men in these categories were part of the "new elite" then surely his sample is inadequate since it appears from table 1 that men from these categories were joining the Society at a rate of only about four per year after 1781. If on the other hand Thackray believes that only the wealthiest and most prominent men from these four occupational categories comprised the new elite, then he must carry his discussion well beyond the McConnells, Kennedys, and Heywoods. He must proceed to demonstrate that there was considerable overlap, if not congruence, between the highest echelon of the new elite and those joining the Society. This he does not do either. Such methodological lapses ultimately make his conclusions about the Society's membership of doubtful relevance to the Mancunian elite, no matter which of the above definitions is used.

Thackray stretches his argument in another direction as well. He claims that social isolation, political impotence, and tumultuous surroundings, among other things, resulted in the necessity of finding social legitimation within the local context and that these factors comprised basic preconditions for the establishment of the Society at Manchester. Yet these preconditions were hardly unique to the period of the early Industrial Revolution. How did social legitimation occur in Manchester before 1781? And did this one Society with its small, restricted membership really accommodate the need for legitimation in a sprawling industrial region?

Part of the answer lies in the fact that the Lit & Phil was merely one social and cultural institution among many. There was theater from the 1750s, concerts from 1744, a Gentlemen's Concert Society of amateur musicians from about 1770, a circulation library in 1756, and prestigious subscription libraries in 1770 and 1792. Social clubs also flourished. These included one in the early eighteenth century for leading manufacturers; the Assembly Rooms, opened in 1752 and rebuilt in 1792, which were notorious haunts for young men desirous of dancing their way into the hearts and fortunes of wealthy and eligible ladies; John Shaw's Club dating from the 1770s, which numbered among its early presidents James Bateman, the famous builder of steam engines, and James Massey, first president of the Infirmary and

also first joint-president of the Lit & Phil; the exclusive Billiard Club, established in 1795, which included representatives from the Peels, Gregs, Philippses, and Heywoods; and the Scramble Club, founded around the turn of the century and made up almost entirely of merchants and manufacturers. The Lit & Phil was by no means first nor necessarily pre-eminent as the sociocultural locus for industrializing Manchester.

There were additional scores of philanthropic, educational, religious, and political committees, clubs, and societies before 1800 in which, through energetic participation, conspicuous largesse, and judicious acquaintance-ship with acknowledged social leaders, prosperous merchants, manufacturers, bankers, and others could hope to achieve social legitimation. Dabbling in science and joining the Lit & Phil comprised only two activities among many that might have helped achieve such legitimation. But while both activities bear mentioning, they hardly warrant the burden of emphasis Thackray has placed upon them.

ROBERT GLEN

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PROFESSOR THACKRAY REPLIES:

I am saddened to see how such strange meanings may be read into my article on "Natural Knowledge in Cultural Context." To cope with all Mr. Glen's arguments would take more space than the issues deserve. It therefore seems best simply to refer your readers back to my presentation (*AHR*, 79 [1974]: 672-709) and let them judge for themselves the saliency of his concerns. By way of example I shall take up only one constellation of objections.

I wrote of my intention to use "the versatile if sometimes barbarous art of prosopography" (p. 678). Mr. Glen deprecatingly points to my employment of the "legitimate, but in some ways limited techniques of prosopography": scarcely a great advance. He then goes on to argue that "it must be proven in each case that an individual truly joined and remained a member [of the Manchester Lit & Phil] in order to adopt science as his cultural mode," and to indict me because I do "not provide this proof" and "probably cannot furnish such proof." Now in my naïveté I had assumed it would strain the patience of readers, to say nothing of the good will of the editor or the resources of the AHA, to pre-empt some hundred of pages in printing the information I

have collected on each of the 588 individuals who constituted the group under study. I had therefore hoped that the provision of systematic statistics, strategic examples and occasional wry anecdotes might give artistic verisimilitude to what, alas, apparently remained a bald and unconvincing narrative.

More seriously, the thrust of Mr. Glen's objections reveal his failure to comprehend the purposes of prosopography or the capacities of cultural history. Certainly, not all the members of the Lit & Phil, let alone all the Manchester bourgeoisie, turned to natural knowledge in this period. Certainly, of those who did, some few were vastly more energetic and influential than the rest. Certainly, other sorts of cultural activity flourished in the town. But to allow the particularistic cataloging of a hundred such matters to obscure the recognition of what was novel and distinctive about those forms of urban-industrial culture which found their exemplar in Manchester is, perhaps, to be a trifle myopic.

ARNOLD THACKRAY
University of Pennsylvania

TO THE EDITOR:

David E. Stannard ("Death and Dying in Puritan New England," *AHR*, 78 [1973]: 1305-30) concludes that the Puritans' manner of dying was at odds with their vision of death, that they were "gripped individually and collectively by an intense and unremitting fear of death, while simultaneously clinging to the traditional Christian rhetoric of viewing death as a release and relief for the earth-bound soul" (p. 1315). This thesis demonstrates a faulty understanding of both the traditional Christian view of death and the Puritan experience of dying.

The Christian view of death is not static. Stannard's "traditional Christian rhetoric" is late medieval, and his discussion ignores the tension in Christian thought explored by Milton Gatch, Jaroslav Pelikan, Oscar Cullmann, and others, between the Greek idea of the immortality of the soul and the Hebraic idea of the resurrection of the body. According to Stannard's "Christian tradition" facile and mechanistic concepts of salvation and assurance allowed optimistic confidence in the face of death. Developing that view he states that the dying man, assisted by family and friends, could "with a little work" and "by resolutely clinging to his optimistic belief in his own goodness and the justness of God" overcome temptation and attain salvation (pp. 1308, 1322).

If all else failed, there was always the chance for a "last minute sacramental reprieve" (p. 1326). First, even when facile and mechanical answers were attempted to the questions of death and salvation, they produced not "Christian optimism" but a loss of confidence in the religious establishment and were a primary cause for the Reformation and its rapid spread throughout Europe. Second, the *Ars Moriendi* actually contradicts rather than supports Stannard's view. Death was not made easy; it remained a great evil. Men in dying, the treatise warned, have the greatest temptations. To meet them required not "a little work" by decedent and friends but the supernatural intervention of a crucified Christ and the saints. The prayers and precepts prescribed for the dying man and his attending friends appeal again and again to God's mercy, never to His justice or to the goodness of the dying person. The "last minute sacramental reprieve," while possible, required repentance, contrition, and faith.

Stannard views death as either reward or punishment while his sources, medieval and Puritan, treat it as both reward and punishment. New England Puritans did indeed find in death "an out-let from sin and misery, and an in-let to *Glory*" (p. 1312). They held it "as fixed," following their mentor John Calvin, "that no man has made much progress in the school of Christ who does not look forward with joy to the day of death and final resurrection" (*Institutes*, III, 9, 5). But concurrent with this optimistic death-as-reward view they also viewed death as the greatest natural evil, the greatest of afflictions, the result of sin, and not originally a part of creation. For these reasons, the Puritans believed that death in itself should be feared. Leonard Hoar wrote that death was "unstung" for Christians, but while the sting of death "cannot poyson them" it could "pierce and pain them" (*Sting of Death*, p. 12).

To find Puritans fearing dying and death is, then, no surprise. Stannard is mistaken, however, in finding this fear to be "unremitting." Elisabeth Kübler-Ross's five stages of dying—denial, anger, bargaining, depression, and acceptance—might not be the best model for early New England, but the idea of stages is imperative. Anxiety followed by assurance was a pattern in Puritan spirituality and their experience of dying, unless cut short, was no exception. While young John Tappin died anxiously and painfully—he was, after all, "not without some hope" (p. 1316)—the equally young and spiritually precocious Nathaniel Mather, after a time of doubting, died peace-

fully (*Magnalia*, IV, 10). Leonard Hoar desired that he might "dye of a consumptive and lingering distemper," not out of hopes for a death-bed repentance, but that he would have time to be fully and consciously prepared for his "great change" (*Sting of Death*, [p. v]). John Eliot, whose dying words were "Welcome joy!" and "Pray, pray, pray!" is an example of a saint who completed the stages of dying (compare to "Communications," *AHR*, 79 [1974]: 917-18). This path to assurance, however, was often cut short by pain and delirium, accompanied by Satan's temptations. The dying fears of Increase Mather, Stannard's primary example, can probably be attributed to this cause since, according to Cotton Mather, "the *Dark Vapours* which assaulted and fettered his Intellectual Powers, broke in upon him" (p. 1315).

What about the peace the rhetoric promised? Whatever troubles saints suffered before their end, James Fitch wrote in *Peace the End of the Perfect and Upright* (1672), if marks of righteousness appeared in their life, then there was no reason to doubt that the Lord would make their end to be peace, if not before the soul left the body, then at least when the angels carried the soul to heaven. Ann Mason's death was Fitch's example of the peace God usually gave to saints. In health she was often "full of spiritual exercise and darksome objections," but in sickness "she had some taste of unspeakable peace and comfort, which in the midst of such pains so disturbing and confounding to the outward and inward senses, yet made her say, she should be at home in a little time, being ready to depart to her Fathers [*sic*] house, and she who had the spirit of adoption could not but under the pains of death, call Abba Father for help" (p. 12).

Stannard's use of sources is questionable in other areas. Many of his "consistant patterns" of godly dying come from Cotton Mather's *The Thoughts of a Dying Man* (1697), a tract expressly designed to awaken men to the fearful possibilities of death for those not fully prepared. It is not a compendium of saintly deaths. The "stout man," for example, whose heart was overwhelmed with dread of death and God's wrath (p. 1317), is identified elsewhere by Mather as a pirate on his way to execution! (*Magnalia*, VI, 5). The need for such a tract and for other contemporary sermons on the Last Judgment and hell consciously designed to arouse indicates that New Englanders, instead of being "gripped by an intense and unremitting fear of death," formed instead an increasingly worldly-minded, bourgeois audience. In

another instance, Stannard truncates a quotation from Cotton Mather (p. 1326) making it rule out the efficacy of death-bed repentance, whereas the full sentence finds such "Change of Mind" to be "usually no more than a conviction" (*The Thoughts of a Dying Man*, pp. 40-41).

The Puritans were, then, traditional in their view of death, a tradition that included a fear of dying itself and an optimistic rhetoric of its outcome for Christians. The fear, however, was not "intense and unremitting," nor was the rhetoric unmindful of assaults by the flesh, the world, and the devil. The tension between the two did not exceed that usually found existing between hope and experience.

GORDON E. GEDDES

University of California,
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TO THE EDITOR:

Perhaps David Donald only skimmed *A History of Mississippi* (*AHR*, 78 [1973]: 1523-25), and felt that a bland positive review would in some way further the cause of historical research within Mississippi. But the book is a—mitigated—disaster, and praising it can only disserve the profession.

From its inception, the enterprise was vitiated by traditional white racism. The state is 40 per cent black; within it live excellent black historians; qualified white historians teach at black institutions within Mississippi; fine black scholars from Mississippi now teach and write elsewhere. Thus it is outrageous that forty-four of forty-four contributors should be white, associated with white institutions.

Predictably, then, the book itself is racist. Bias is shown perhaps most blatantly by the editor himself, writing on higher education in this century. Dr. Richard McLemore ignores almost every major issue, particularly of the past twenty years, instead listing such "facts" as the names of the presidents of each white college. He makes no mention of Mississippi's "unwritten rule" against participation by white schools in events that might be integrated, nor of Mississippi State's violation of it in 1963. The clampdown on academic freedom at places like Millsaps and Mississippi State, 1954-64, goes unreported. His description of the formal desegregation of higher education, which he terms "integration," ignores the fact that desegregation has not really occurred and gives the reader no basis for understanding why new federal orders now face these institutions. Mc-

Lemore even engages in the petty racism of listing the institutions unalphabetically so he can put the black schools last!

This theft of the past for public-relations purposes denies to all Mississippians a resource for understanding the present. And the book's errors are no accident. For example, when Dr. Bettersworth uses 1890s oral interviews to debunk the "free state of Jones" events, against better evidence to the contrary, he does so in order to expunge from state history the fact that there were at times whites who dissented from the racist policies of the state's white leadership. Such deletions destroy the heritage of those whites who would find dissenting heroes in their history with whom to identify.

Likewise, the lack of full biographical treatment of even one black Mississippian destroys the heritage of black Mississippians so far as this work is concerned.

Another kind of racism pervades the better chapters of the book. The chapter on civil rights is an example. Dr. McMillen has based his chapter on his fine book on the Citizens

Councils. It is not, therefore, a history of the black struggle for civil rights, but of the white reaction to that struggle. Even here, then, blacks do not act. The Reconstruction chapters similarly focus upon such matters as the bills passed by the legislature and do not get into the massive changes occurring in black institutions and social life.

There are two good chapters—by Moore and Bearss. Some other chapters have something to offer, but overall the work is very poor. Dr. Donald's review is simply far too glowing. His own acceptance of the idea that history should be racist is shown by the following sentences from his review: [these essays] "offer most readers all the information they ever need to know about Mississippi"; and "references to the black half of the state's population are, throughout, few." His review, along with the book itself, convince me that we simply cannot expect historical scholarship about Mississippi from older white Mississippians.

JAMES W. LOEWEN
Tougaloo College

Recent Deaths

JESSE DUNSMORE CLARKSON, a native of Brooklyn, New York, died in Bay Shore, New York, on September 5, 1973, at the age of seventy-six. He spent his undergraduate years at Williams College (B.A. 1918) and, under the sponsorship of Carlton J. H. Hayes, received his doctorate at Columbia University in 1925. He began his teaching career at City College of New York. In 1930 he joined the faculty of newly established Brooklyn College where he remained until his retirement in 1967. In the 1950s and 1960s he repeatedly served as visiting professor of Russian history at Columbia University and in the same capacity at Berkeley in 1962.

Clarkson was one of the founders of Russian historical studies on this side of the Atlantic. This notable achievement was not prefigured by his erudite and imaginative doctoral dissertation published under the title *Labor and Nationalism in Ireland* (1925). This massive work gives a detailed, problem-oriented account of the history of the urban labor movement, both as a trade-union and a political movement, by penetratingly correlating the successive stages of labor's revolt against the established order in Ireland with the perennial struggle for national independence.

The mid-1920s marked a crucial turning point in Clarkson's scholarly activity. Uncommonly enough, in lonely self-reliance and in deviation from current academic practices and interests at a time when American research on Russian history was almost equal to zero, he boldly made a fresh start by concentrating his energies thenceforth on the methodical pursuit of the study of the Russian past. The first visible fruit of this historiographical reorientation was the translation and editing of a Marxist classic: M. N. Pokrovsky's *History of Russia from the Earliest Times to the Rise of Commercial Capitalism* (1931). Thirty years later Clarkson presented his own interpretive version of more

than a thousand years of Russian development in *A History of Russia* (1961).

A result of wide reading, persistent critical probing and sober reflection, this huge work, rich in colorful information, demonstrates Clarkson's analytical acumen, the clarity and precision of his thinking, the independence of his views, and the forcefulness of his incorruptible, sometimes caustic and provocative judgment. Though unevenly balanced in temporal and topical coverage and somewhat contrived in the portrayal of Russian culture, his volume is nevertheless held together by invigorating unity of thought cemented by a sharp grasp of the practical realities of historical life. Suspicious of personalistic and nonmaterialistic interpretations of historic continuity and change, Clarkson's central effort focuses on the process of the interplay of impersonal economic, social, and political forces and on the understanding of the web of structural relationships.

Clarkson had a highly intriguing, complex personality. This, blended with his learning, dedication, and unusual dialectical powers, accounts for his remarkable success, on various levels of discourse, as an inspiring teacher. As an administrator and academic community leader he was stubbornly committed to the safeguarding of exacting standards of performance. At Brooklyn College he exerted a decisive formative influence on the history department, most strikingly during the years 1937-50 when he served as its chairman.

Jesse D. Clarkson was a man of stature who will leave a mark in our profession.

HANS ROSENBERG
*University of California,
Berkeley*

The sudden death of BEATRICE FRY HYSLOP in Rochester, New York, on July 23, 1973, deprived the *American Historical Review* of a distinguished contributor of articles and re-

views. For the last quarter of a century, moreover, she compiled its listing in French history for the Recently Published Articles section.

Born in the city of New York on April 10, 1899, the daughter of James Hervey and Mary Fry Hall Hyslop, she was educated in public schools and Barnard School for Girls, at Mt. Holyoke College (A.B. 1919), and at Columbia University (A.M. 1924 and Ph.D. 1934). Her father was a philosopher, psychologist, and leader in psychical research. Miss Hyslop taught briefly at Mt. Holyoke College from 1926 to 1928 and in 1936 was appointed instructor at Hunter College, where during the next thirty-three years she progressed steadily and retired with the rank of full professor. The opportunity to direct professionally oriented students did not come until late in her career, but she was a member of the graduate faculty of the City University of New York from its formation in 1961 until her retirement in 1969.

Her historical curiosity and her industry were prodigious. She gave of her knowledge and experience constantly and generously to students at all stages of their intellectual development; and throughout her life her articles, lectures, and reviews found place in major historical journals at home and abroad. Her interest in European history and the history of art had begun in college, and at Columbia she focused that interest on French history and began the researches in the French materials that were to establish her firmly as an authority on the *cahiers*. Even before she received the doctorate, the thoroughness of her method and her familiarity with French archives had been indicated by the publication of her *Répertoire critique des cahiers de doléances pour les états-généraux de 1789* (Paris, 1933; *Supplément*, 1952). This and her doctoral thesis, *French Nationalism in 1789 according to the General Cahiers* (1934) were quickly followed by the *Guide to the General Cahiers of 1789* (1936). Two of her major writings came later: *L'Apanage de Philippe-Égalité, duc d'Orléans 1785-1791* (Paris, 1965) and a textbook, in collaboration with Jacques Godechot and the late David Dowd, *The Napoleonic Era in Europe* (1970). At the time of her death she had in preparation and under contract a new study of the *cahiers*.

As a woman of untiring friendliness and as

an active member of learned societies on both sides of the Atlantic, notably the American Historical Association, the Society for French Historical Studies, the Société des études robespierristes, and the Société d'histoire moderne, she contributed in an extraordinary degree to the promotion of Franco-American historical cooperation. Her position was recognized in various ways. In 1951-52 she was awarded a research fellowship from the Fulbright Foundation; and in 1955-56 on a Fulbright exchange professorship she lectured at the École des Hautes-Études of the Sorbonne and at the University of Toulouse. The government of France decorated her as Chevalier des Palmes académiques in 1931 and raised her to Officer in 1952, and in December 1961 invested her as Chevalier de la Légion d'Honneur. The review *French Historical Studies* paid her the unprecedented tribute of dedicating to her its Fall 1972 number; and the parent society in full meeting at Chapel Hill in March 1973 presented her with a specially bound copy of the issue. In her honor the Ash Avenue Unitarian Universalist Church in Jackson Heights held a memorial service on July 31, 1973.

GEORGIA ROBISON BEALE
Orford, New Hampshire

Other members of the association who have died recently include: James E. Bland of Bowdoin College, Brunswick, Maine; Henry M. Dater of Washington, D.C.; Stanton Griffis of New York City; Clinton N. Howard of the University of California, Los Angeles; J. E. Jordan of California State College, Fullerton; Suzanne G. Korirsh of Redwood City, California; Jakob Aall Ottesen Larsen formerly of the University of Missouri at Columbia; Murray G. Lawson of Washington, D.C.; Georgiana Putnam McEntee, professor emeritus of history at Hunter College, New York City; Robert D. Meade of Randolph Macon Woman's College; Richard Lee Morton of Williamsburg, Virginia; Kenneth Munden of Arlington, Virginia; Bessie Louise Pierce, professor emeritus of history at the University of Chicago; Heston N. Potts of North Branch, New Jersey; E. C. Rozwenc of Amherst, Massachusetts; Albert K. Weinberg of Baltimore, Maryland; and Edward F. Wenz of Cincinnati, Ohio.

Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

BOTZ, GERHARD; HAUTMANN, HANS; and KONRAD, HELMUT, editors. *Geschichte und Gesellschaft: Festschrift für Karl R. Stadler*. Vienna: Europa-Verlag, 1974. Pp. 583. DM 45.

Geschichte der Habsburger-Monarchie: ERICH ZÖLLNER, Aus Stammbüchern österreichischer Handwerker. HEINRICH LUTZ, Politik und militärische Planung in Österreich-Ungarn zu Beginn der Ära Andrassy. Protokoll der unter Allerhöchstem Vorsitz am 17. Februar 1872 abgehaltenen Konferenz. (16^{te}) Protokoll der unter dem Allerhöchsten Vorsitz am 18. Februar 1872 abgehaltenen Konferenz. RICHARD GEORG PLASCHKA, Widerstand 1915 bis 1918 am Modell Pilsen. Ein Industriezentrum der Donaumonarchie im Spiegel der Berichte der Zivilbehörden.

Österreich 1918–1938: GUSTAV OTRUBA, "Bauer" und "Arbeiter" in der Ersten Republik. STEPHAN VEROSTA, Bemerkungen zum Brief Otto Bauers an Jean Longuet vom 9. Januar 1919. ERIKA WEINZIERL, Aus den Notizen von Richard Schmitz zur österreichischen Innenpolitik im Frühjahr 1933. LUDWIG JEDLIČKA, Gauleiter Josef Leopold (1889–1941).

Zweite Republik: ADAM WANDRUSZKA, Österreich und Italien seit dem Zweiten Weltkrieg. RUDOLF STRASSER, Der Beitrag des Obersten Gerichtshofes zur Arbeitsrechtsentwicklung in der Zweiten Republik. FELIX KREISSLER, Die Entwicklung der SPÖ in ihren Programmen und in ihrer Politik: Vom Austro-Marxismus zum "Austrosozialismus" (1945–1973).

Geschichte der Arbeiterbewegung: ERNST WANGERMANN, Die Auseinandersetzung über das Verhältnis von Reform zu Revolution in der deutschen und österreichischen Sozialdemokratie. FRANCIS L. CARSTEN, Arthur Rosenberg als Politiker. ADOLF STURMTHAL, Werner Sombart und der amerikanische

Sozialismus. YVON BOURDET, Georg Lukács im Wiener Exil (1919–1930). GEORGES HAUPT, Renaissance oder Stagnation des Marxismus heute?

Probleme der Industriegesellschaft: FRIEDRICH FÜRSTENBERG, Angestellte und Gesellschaft. GERHARD WEISSER, Der unkontrollierbare Apparatschik. CHARLES A. GULICK, The Fight for San Francisco Bay: The First Ten Years. HERBERT SCHAMBECK, Der Staat und die Demokratie. EDUARD MÄRZ, Klassengesellschaft und Pluralistische Gesellschaft.

Schule und Erwachsenenbildung: ROBERT A. KANN, Hochschule und Politik im österreichischen Verfassungsstaat (1867–1918). HAROLD C. WILTSHIRE, The International Contribution of British Adult Education. KURT W. ROTHCHILD, Bildung, Bildungspolitik und Arbeiterbewegung. WILLY STRZELEWICZ, Die Funktion der Schule in der gegenwärtigen Gesellschaft.

Conjoncture économique, structures sociales: Hommage à Ernest Labrousse. (École pratique des hautes études—Sorbonne.) Paris: Mouton, 1974. Pp. 547. 140 fr.

Orientation générale et méthode: PIERRE CHAUNU, Conjoncture, structures, systèmes de civilisations. PIERRE VILAR, Réflexions sur la "crise de l'ancien type": "Inégalité des récoltes" et "sous-développement." JEAN BOUVIER, Feu François Simiand? GEORGES DUPEUX, L'étude de la mobilité sociale. Quelques problèmes de méthode. LOUIS BERGERON, Histoire sociale et histoire urbaine. À la recherche d'une méthode. F. G. DREYFUS, A propos du Kathedersozialismus. CLAUDE FOHLEN, L'expérience américaine. Un cas unique?

Conjoncture économique: PIERRE JEANNIN, Conjoncture et production du cuivre dans les Vosges méridionales à la fin du XVI^e et au début du XVII^e siècle. BARTOLOMÉ BENNASSAR, Consommation, investissements, mouvements de capitaux en Castille aux XVI^e et XVII^e siècles. CHARLES CARRIÈRE, La draperie languedocienne dans la seconde moitié du XVIII^e siècle. Contribution à l'étude de la conjoncture levantine. JEAN GEORGELIN, Le mouvement saisonnier des prix du froment et du maïs à Pordenone (fin XVIII^e—début XIX^e siècle).

MICHELLE PERROT, Comment les ouvriers parisiens voyaient la crise d'après l'enquête parlementaire de 1884. FRANÇOIS CARON, La grève des cheminots de 1910. Une tentative d'approche. PIERRE BARRAL, La soudure du blé en France en 1942 et en 1943.

Structures et conjoncture: FRÉDÉRIC MAURO, Conjoncture économique et structure sociale en Amérique latine depuis l'époque coloniale. VITORINO MAGALHÃES-GODINHO, L'émigration portugaise du XV^e siècle à nos jours. Histoire d'une constante structurale. JACQUES DUPÂQUIER, Les mystères de la croissance. Soixante-trois paroisses d'Ile-de-France de 1717 à 1784. FRANÇOIS CROUZET, Recherches sur la production d'armements en France, 1815-1913. MARCEL GILLET, Structures et conjoncture: Houille et métallurgie en France au début du XX^e siècle.

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EASTERN EUROPE

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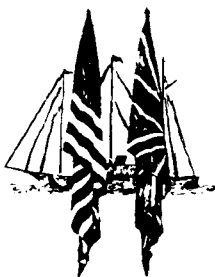
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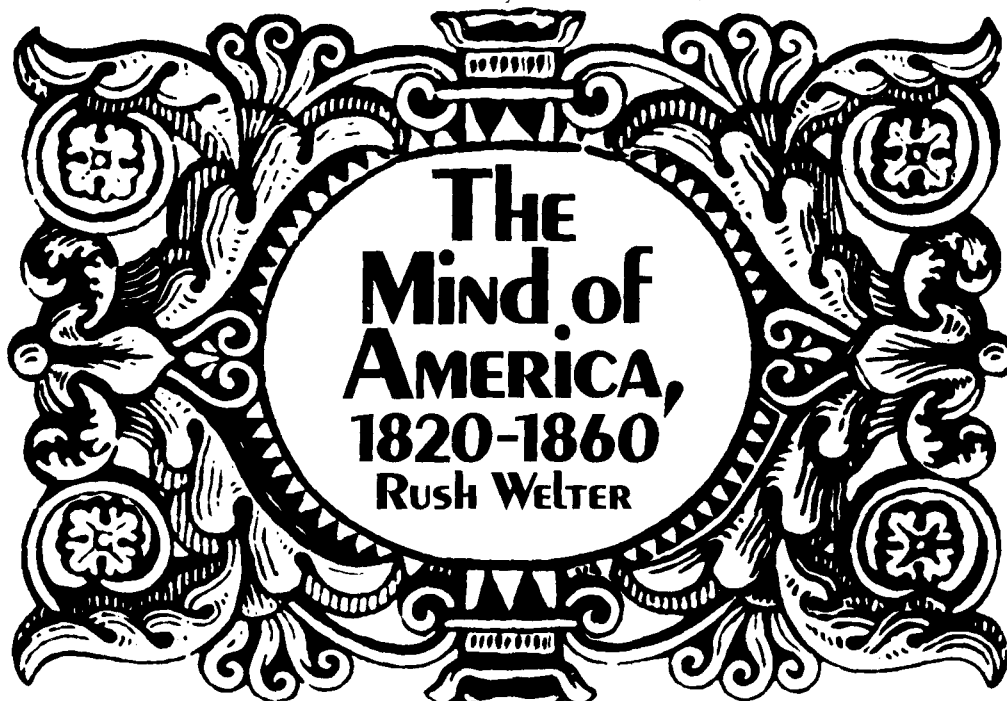
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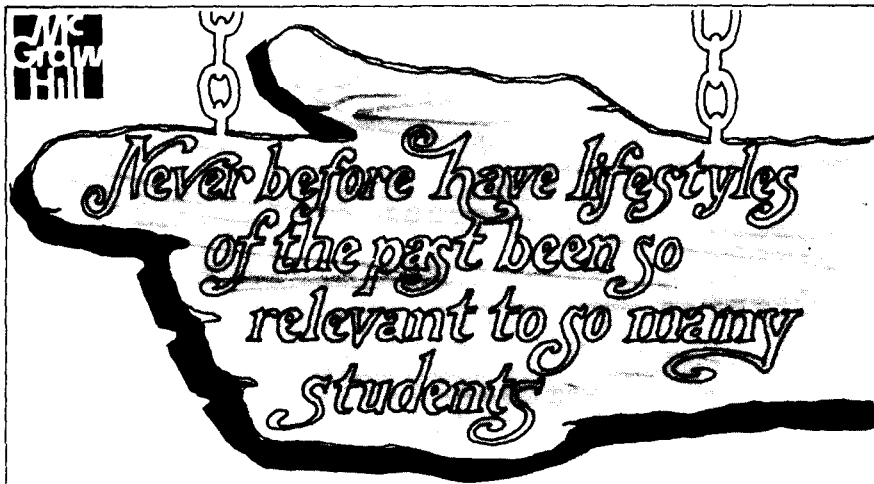
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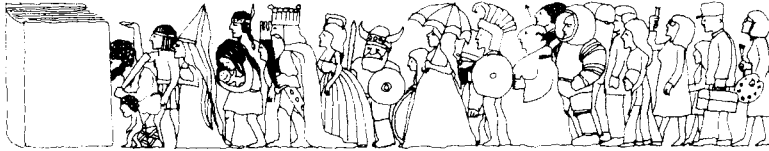
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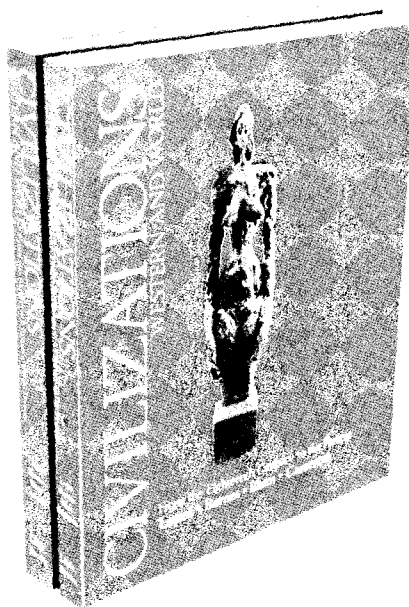
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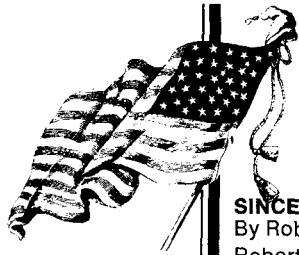
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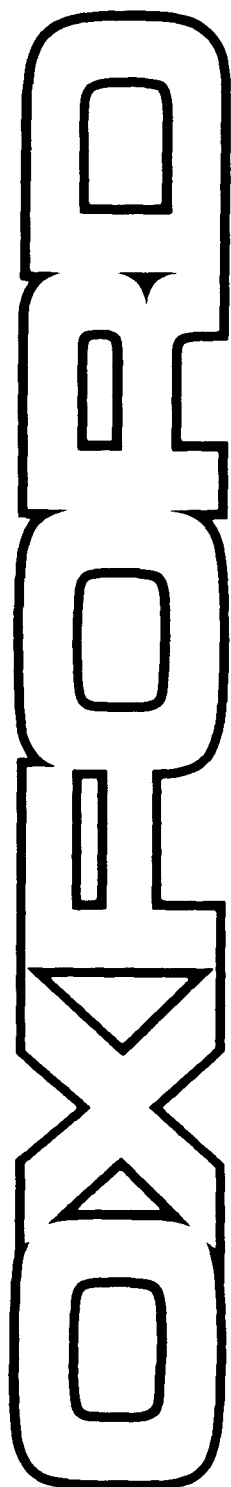
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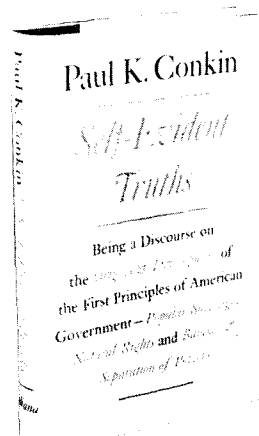
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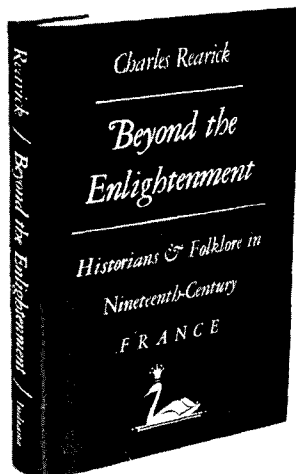
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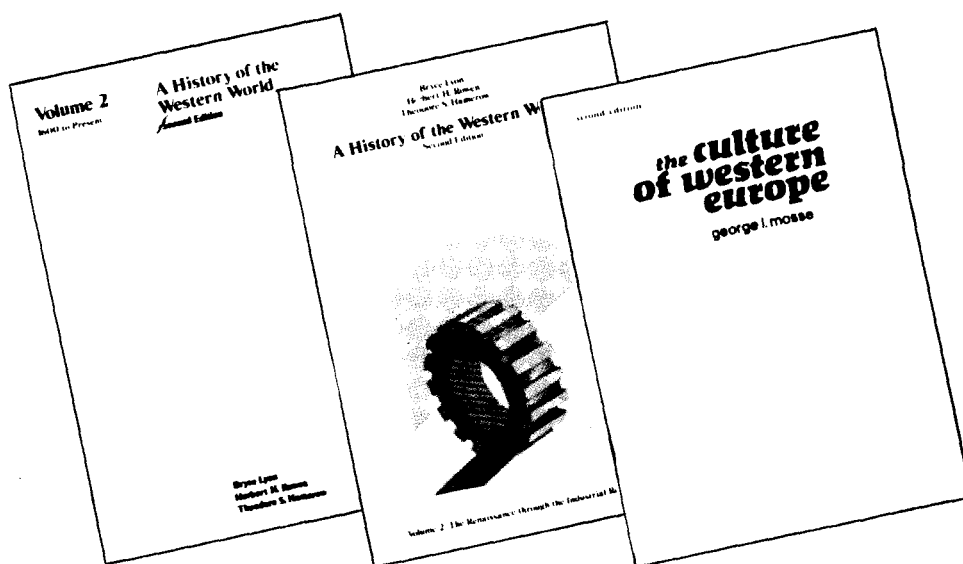
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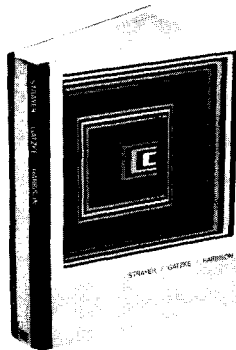
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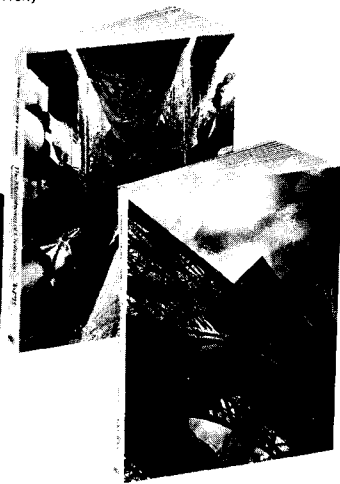
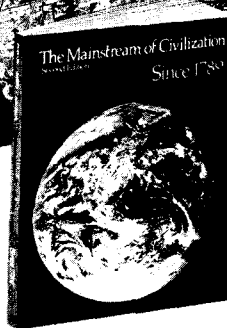
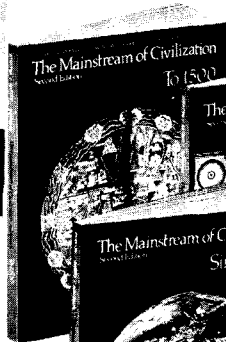
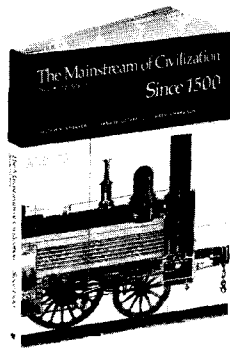


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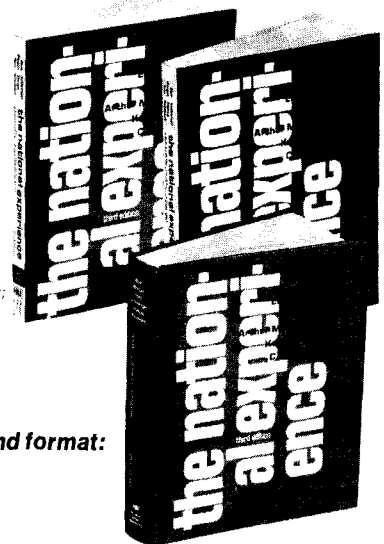


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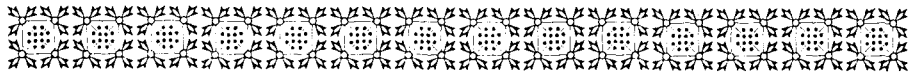
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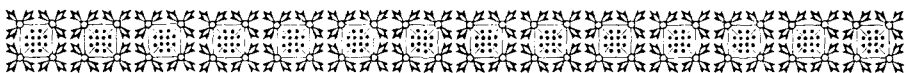
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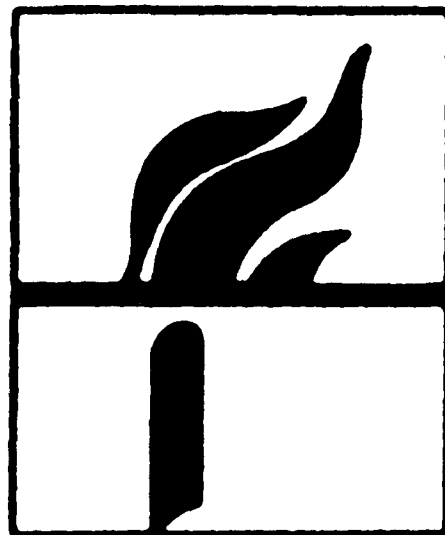
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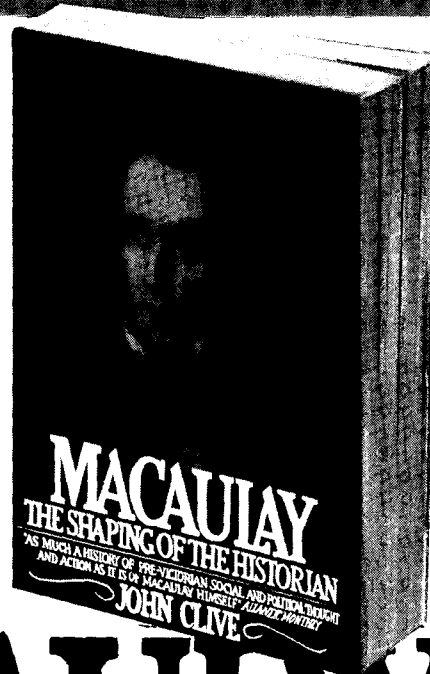
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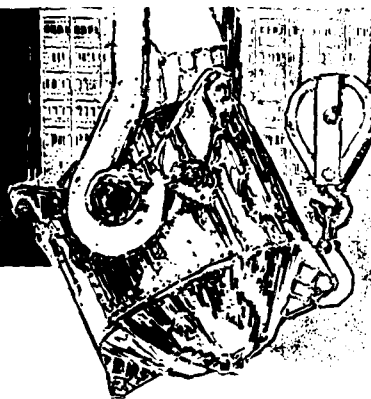
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